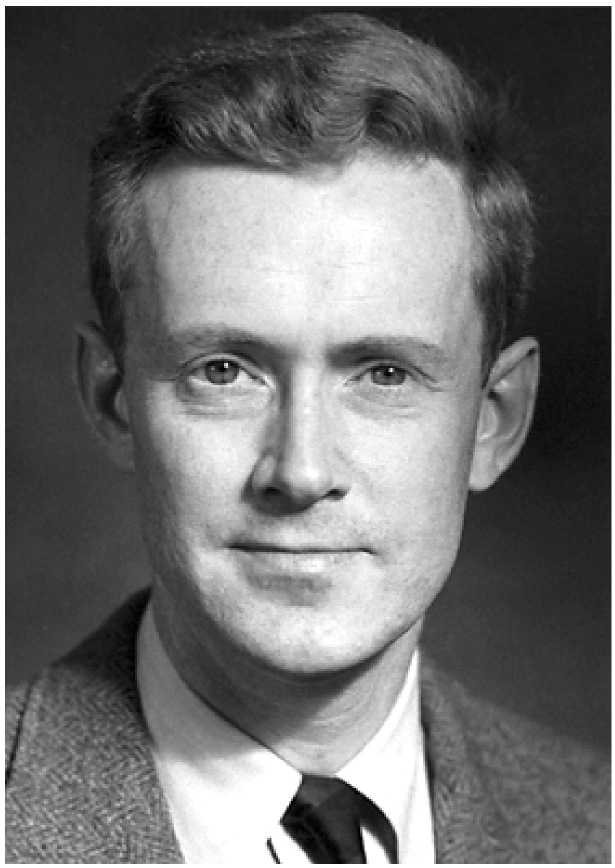
# Interviews with Prof. Edward Purcell



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# Interview Session - 1

**Sopka:**

This is Katherine Sopka. I'm visiting today, November 23rd, 1976 with Professor Edward Purcell in his office in the Lyman Laboratory. In the interest of compiling a history of the Harvard Physics Department in recent decades, Professor Purcell has kindly consented to share with me his recollections of trends and events that have shaped the course of that history since his arrival as a graduate student in 1934. Professor Purcell, perhaps we can begin by asking you to recall your impressions of life within the department when you first came.

**Purcell:**

Well, the contrast with today that stands our most sharply when I try to think back is how serenely uncrowded the place was in those years starting in '34 with me. I was of course very green, and never having been at a place like Harvard before and having studied engineering rather than physics mostly before, although I'd just had a year in

Germany in physics, so I saw the department very much from the worm's eye view, but still there weren't very many students. As I remember there were perhaps 20 or 25 graduate students. The classes were small. The professors were on the whole I thought very, very kindly and really there were some outstandingly kind people — Saunders and Oldenberg come to mind from my first year. I had courses with Kemble, a great teacher, although that wasn't the first year and Chaffee. I remember that when it became time to find a thesis topic I had a room, a whole research room, all to myself even before I had a topic. The same room now is just below this room here. It's crowded with apparatus and graduate students. I remember that the picnics in those days were rather congenial, friendly affairs. Somehow the intense pressure that's on the present graduate student didn't seem to be so acute. It wasn't that there were a lot of jobs, it's just that there were I guess fewer people. Some of my most intense intellectual experiences at Harvard were actually — one of them was in mathematics where I for the first time got a glimpse of what mathematics really is by taking Math 13 as it was then called. And of course the university was a very exciting place for a boy

from Purdue, and I remember going to hear, as an auditor, Whitehead's lectures on cosmology and things like that. And we lived in the dormitories and I lived in Conant Hall and Perkins Hall at various years. There were no eating facilities whatever for graduate students, so we took our meals up and down the various little restaurants on the northern stretches of Massachusetts Avenue. It was a very, really a rather — jolly is perhaps not quite the right word, but it was a, there was a good fellowship among the mathematics and physics students in the halls. I set out to do my thesis. Well, first Professor Chaffee gave me a thesis topic. I had taken his course in electron physics the first year I was here I think. He gave me this thesis topic which actually would have been a superb topic. Indeed if I had had the sense to stay with it, it would have resulted in the invention of what was later called the omegatron about ten years ahead of its time. Chaffee had the idea that it might be interesting to run a gas discharge in a magnetic field and then measure the impedance of the discharge with an RF our bridge and maybe you would see something funny happening at the cyclotron frequency of the ions in the gas, and if it did, well then you could obviously

have a mass spectrograph made that way. And so I started to get the apparatus together to try this, and I had the apparatus really pretty well assembled — at the least the coil for the magnetic field and a bottle for the discharge. And I started doing a lot of calculations and convinced myself that it couldn't work, and I took the calculations around to Chaffee and showed him and the argument was that the ion wouldn't stay in the field long enough to have it display any resonance before it had collisions and things like that. And Chaffee was persuaded by my calculations, and so we dropped the thing, which was really a great pity, because it was in fact a good idea and if I'd only pursued it a little farther I would have been quite likely to stumble on a method to eliminate the difficulty with the lifetime. And in fact it wouldn't have taken very much inspiration starting from Chaffee's idea to invent the omegatron. So then I, looking around again, did some experiments at the suggestion of Bainbridge and finally ended up with a thesis topic that he had suggested which turned out to be doable and interesting, and I did it. But that a graduate student could shop around like that in those days and have a room to himself while he tried out various ideas is in rather sharp contrast to the

situation today. The courses were small. I, of course, took Van Vleck's electric and magnetic susceptibilities course which was the course he gave based on his marvelous book. And Malcolm Hebb and I were the only registered students in the course. I think there may have been one or two auditors. But Van lectured to us for a semester, and then he assigned a term problem to work on at the end of the semester which Malcolm and I literally spent the next year working on, and which turned into be really one of the best papers I've ever had my name connected with. And I remember we took the final exam, and in those days the final exams of course were printed, set up in type and printed on a printing press, and of course serially numbered. Malcolm and I had serial number 1 and serial number 2. I often wondered how they stopped the presses before they turned out a very large excess of copies of that examination. But that's what made it really such a wonderful time for a graduate student, with Van and Kemble, and then Bridgman of course, a very great man from whom I had thermodynamics, my third course in thermodynamics actually. The first one was taken at an engineering school and was an absolutely travesty on the subject. The next one in

Germany which was a really eye opener, and finally Bridgman's, which was an eye opener in many other ways. Well the cyclotron then took over our interests toward the end of the '30s, and many of us worked on that, building it.

**Sopka:**

Professor Street told me yesterday that you were responsible for one particular development which helped get the cyclotron in operation.

**Purcell:**

Well, let's see how it was. My job was to worry about the magnet, the power supply and the control circuit for the big cyclotron magnet — well, big in those days. It doesn't seem so big now. And I built the control circuit for that with other people's help, and we all worked over there putting in wiring and things like that. I then, after the cyclotron got running, proposed and developed an innovation which was an automatic tuning of the cyclotron which was a rather Rube Goldberg kind of system which modulated the radio frequency by a rotating capacitor plate which its vicious whine really scared people in the vicinity, and this thing changed the

frequency of the cyclotron up and down a little bit, and then we looked at the modulation of the beam current and used the phase of that modulation to control the main generator indirectly by controlling the field on the exciter generator. It was a negative feedback system in the days when such were not well understood by most people, especially including me. There may have been some people in those days who understood the stability of the feedback system but I was not one of them. But I finally got it working by stuffing in enough RC circuits here and there to keep the thing from, uh, to make it stable. And it worked. It automatically tuned the cyclotron, which it was actually enormous help. But it was a lot of fun. I mean, I look back on that cyclotron building period. It's certainly one of the most interesting episodes in my professional life, learning so much and I remember when we finally got the beam out and how hard it was. Of course we were copying Berkeley, as everyone did. Our machine was a slightly scaled down version of the then Berkeley machine, and we were following them, using their techniques, including the Berkeley shims that you had to shove around to try to get the beam to come out. But it was quite a good machine,

thanks in no small part to Ken Bainbridge's very careful engineering. Of course like all cyclotron workers in those days, we were in retrospect ridiculously careless with radiation. But fortunately I think no one in the Harvard cyclotron suffered any ill effects such as cataracts that other people occasionally got. We let the beam come out in air and take pictures of the curing deuteron beam and air and stuff. Well, it was very shortly after that that I vanished from Harvard into the MIT Radiation Laboratory, so that there is a period at the end of 1940 to the end of 1945 when I practically didn't set foot in the laboratory here, and of course in that period, the wartime period, there were — the activities here were mainly taken over with training naval electronics people and things like that. Weren't they?

**Sopka:**

Yes. There were almost no advanced courses given. It was all army and navy STP programs. There were a number of Harvard physicists down at MIT though in the Radiation Lab?

**Purcell:**

Oh yes. There were, well, let's see if we can —

**Sopka:**

Or people who have come to Harvard since.

**Purcell:**

Well, the people who went down there from Harvard included of course Bainbridge and Street. Somewhat later Furry, who was in the theoretical group at the radiation lab starting about '41 — maybe '42, I'm not sure. Let's see, who else? Jack Pierce was there working with, who had been working with Mimno and Pierce and G. W. Pierce here. Working mainly with Mimno, Jack Pierce was down there in the Loran business with Street. Bill Preston was at the Radiation Lab, and then lots of people who were here after that — Schwinger and Ramsey and Pound. Others I guess. I don't remember what happened to all the cyclotron people. Jack Livingood was one of the people on the Harvard cyclotron; Curtis, Biggs Curtis [?]; and Ruby Scherr. Scherr came to Radiation Labs. That's right. I guess and later went to Los Alamos, as did Norman Ramsey and

Bainbridge. Well at any rate, as far as I was concerned I didn't know what was going on here at all, although there was the so-called Radio Research Laboratory at Harvard which absorbed other Harvard people, including Van Vleck.

**Sopka:**

Yes. I believe the Radio Research Laboratory was concerned with the anti-radar.

**Purcell:**

Yes. That was the radar countermeasures as it was called, Felix Bloch was there, and it was organized under Fred Terman. We had rather little to do with that, because since they were doing countermeasures they were sort of one level higher security and they were supposed to know what we did, but we weren't supposed to know what they did, more or less. But as it happened, my work at Radiation Lab had very little connection with countermeasures, so I paid almost no attention to the Radio Research Lab up here.

**Sopka:**

Did you feel that the work you did at MIT during those years significantly contributed to your own scientific development, or was it a hiatus in your —?

**Purcell:**

No, no, no, it was tremendously — in fact everything I've done since, everything since radiation lab in my professional activity almost directly stems from the influence of the work there.

**Sopka:**

That's very interesting.

**Purcell:**

Not only the work, but actually the people. That is, I spent most of the war working in groups that were with whom Rabi was closely associated. In fact he was the nearest thing to my boss through the Radiation Lab was I. I. Rabi, and I worked very closely with people who had come up through Rabi's laboratory like Ramsey and Zacharias. And I also worked with the people who were at Columbia during the war in the Columbia radiation laboratory

— Kellogg and Kusch and Lamb and Nordsieck, so that I — well, see the two most powerful influences on my own subsequent work were getting to know, being immersed in the physics attitudes of people from the Columbia, from Rabi's Columbia laboratory, which necessarily made you, stimulated you to think about resonances of one sort or another and how you could detect them. And then of course getting to know all the new microwave electronics techniques, particularly microwaves and problems and questions of signal and noise and things like that which were crucial in all experiments that I was going to be doing in the next several years. You see, the point was that in the radar business we had to understand for the first time for most of us the general signal-to-noise problem. How do you go about detecting a weak signal and what in principal is the weakest signal you can detect and so on, so that we were able to calculate on the back of any envelope just how we would go about detecting an effect of a given strength. And all the postwar magnetic resonance experiments for example, whether they were by ourselves or other people, hinged on that as did the experiments in radio astronomy, including 21-centimeter observation.

That all just really traces back very directly to what we learned in working on radar. The whole idea of wave guides we picked up there, absolutely new to most of us. When we went to radiation lab, we had no idea about things like that. Even though I was trained as an electrical engineer in the early '30s, that stuff was completely new to me because, in my training we had never talked about lossless transmission lines, let alone wave guides. And then there was wave propagation, and that came into radar in a very important way, the kind of thing that Furry worked on through the war. It was a tremendous education that we were fantastically lucky to come out of it with that great advantage.

**Sopka:**

In the case of radar research during the war, I assume that people like yourself could see the total picture in a different way than the people who were on the Manhattan Project where unless you were quite high up in the project you only saw a very small portion.

**Purcell:**

I think that's true. Certainly in the radar business there were no internal compartments at all, only those that just made by a lack of time and concentration on one problem at the exclusion of another. But we were very close to the applications from the beginning in contrast to the Manhattan District people. That is to say, radiation lab people were working very closely with the military and we were, apart from that we were very close to the actual war because of our close association with our British colleagues who were in the thick of it, so that the very first months of Radiation Lab we had people fresh from the battle of Britain which was then going on telling us what the problems really were in trying to shoot down bombers with night fighters for instance. Later in the war there were branches of Radiation Lab in Europe — in Britain first, and then in France after D-Day.

**Sopka:**

Did you go to the Radiation Lab before Pearl Harbor Day?

**Purcell:**

Oh yes, oh heaven's yes. No, the Radiation Lab, I went to radiation I think in December 1940.

**Sopka:**

I see. A whole year ahead.

**Purcell:**

A whole year ahead. And no, our first job at Radiation Lab was to try to make a microwave radar for a British night fighter using the British 10-centimeter magnetron, which had been invented at Birmingham by Oliphant and Boot and Randolph and brought to this country by the Tizard mission and successfully copied at Bell Telephone Labs by Jim Fisk and his group there. So that was our start in radar. We ended up with many other things. Of course the lab ended up with four thousand people, but when I went down it was just, oh, I guess 30 people or so in a room, big room [laughs] with this one job to focus efforts on, although inventing a lot of things, because half the things that were required we had no idea how we were going to do. Then I stayed rather long at the Radiation Lab because I

was one of the large group that stayed to write the books, Radiation Lab series, and I think there even was a period when I was sort of half time back at Harvard and half time down there trying to finish the book writing. But in any case we came back to Harvard to do the NMR experiment.

**Sopka:**

Was it clear that you were coming back to Harvard all the time that you were at MIT, or was that something to be renegotiated?

**Purcell:**

Well, my Harvard rank was faculty instructor I believe, on leave. It was a time for a few years when the faculty had abolished — the rank of — what was it?

**Sopka:**

Assistant professor?

**Purcell:**

Assistant professor. That's right. There was no assistant professor. You were a faculty instructor, which was a 5-year appointment, I believe. But

before I actually came back I remember being offered a permanent appointment, because I remember that Ted Hunt and somebody else — maybe it was Street, no I think it was Ted Hunt came down to see me at lunch at Radiation Lab and I had seen him — He was of course involved in the underwater place called you know the sound lab or whatever it was here, the sound laboratory. He was director of it. So he came down to tell me that, to ask me if I would be interested in coming back to Harvard as an associate professor, to which I said I was, and that was all there was to it. I can't remember, can't date that but it must have been, I think that must have been the summer of '45. I think that had been settled at the time when I came back and we did the nuclear resonance experiments here, I think otherwise I would not have been quite as — not have been so free with coming back and using this stuff and getting things done in the shop. So I think that's probably right. We came back and saw Curry and borrowed the magnet — the old cosmic gray magnet for that purpose. And started that work in the fall of '45. Although all of us that by that I mean Pound and Torrey and myself were still Radiation Lab employees. In fact our resonance

cavity that we did the first experiment in was made in the Radiation Lab in the machine shop. Our first publication lists us as from Radiation Lab not from Harvard.

**Sopka:**

I see.

**Purcell:**

In fact I tried to borrow a magnet down at MIT and did not succeed. That's why we had to come back and do it here. We would have done it down there if somebody had given us —

**Sopka:**

They agreed to make your cavity but not to let you use —?

**Purcell:**

No, they had agreed to make our cavity. It was just made

**Sopka:**

Oh. Unbeknownst.

**Purcell:**

Well, the IF group, the machine shop, yeah, they were accustomed to making cavities and Pound just put in a drawing and had it made. So that they had the guy make that cavity. I met the fellow just a week ago that made that. We had a big Radiation Lab reunion at MIT.

**Sopka:**

Oh, I read about that, yes.

**Purcell:**

Yeah. And this fella came up to me and he said, "You know, I made that cavity." He was a machinist in the Group 53 shop that made it.

**Sopka:**

How many people came to that reunion?

**Purcell:**

And I told him it was at the Smithsonian now. He could go see it if he wanted to. Over three or four hundred people. Yeah. It was an interesting affair. Rabi was there, and DuBridge, Killian and Jerry

Weisner [?] of course, lots of the old fellows came there. H. Guiford Stever [?].

**Sopka:**

Oh. NSF.

**Purcell:**

Bob Dickey. A lot of people I hadn't seen for a long time. Getting kind of old. Well when we came back to Harvard after the war, of course for me it was a very exciting time, because we were into nuclear resonance and things were really popping. Bob Pound came back with me as a Junior Fellow, and we were all full of the microwave stuff and everything, and then of course at the time in nuclear physics the whole world was opening up in a most exciting way also, so that there were so many things to do and teach. The Mark I computer was here and then it left — It chugged away through most of the war here, didn't it, the Mark I?

**Sopka:**

I believe it began operation during the war.

**Purcell:**

During the war. That's right. It spent most of its time doing Bessel functions order one-third, and there was a good reason for that. One of the unexpected phenomena in microwave radar was called trapping, which was the radar waves normally which would go in straight lines and therefore could not see a ship around the horizon would sometimes see around the horizon for a hundred and two hundred miles, owing to, as it turned out, the fact that the lower few hundred feet of the earth's atmosphere over the ocean has a gradient of water vapor in it, and this makes a gradient of refractive index which is capable of trapping and guiding a wave along the surface. And the theory of this, which Wendell Furry worked on, is one of the leading workers on developing the theory of this thing; propagation in a duct like this involves Bessel functions order one-third. And since the navy owned the computer, at least Howard Aiken was by then the commander, and the navy had purview of the ocean and used radar on it. Bessel functions order one-third were a high priority item. Of course it was before that the real age of computers in physics when we came back. Well we came back to find that the ionosphere

research, which had been quite important at Harvard in the '30s under Mimno and Pierce working under Mimno, was still going on, and this was rather distressing because Pound and I were trying to do nuclear resonance experiments here in Lyman Laboratory which required fairly good receivers and keeping the noise down and all that, rather delicate measurements electrically, and the antennas up on top of the laboratory were sending up these microsecond pulses to the ionosphere, you know, which were just making a noise all over the spectrum. And after suffering this interference for quite a while we finally got enough clout (as they would say now) to cause Mimno to move his show out into the country.

**Sopka:**

I see.

**Purcell:**

And turn off those terrible pulses that were smearing ink all over the estolen [?] and angus [?] record. It was certainly an exciting time, those years, which were of course also the years when the graduate school here was expanding, and of course also the

years when those of us who came back as young teachers full of enthusiasm and everything, had the marvelous experience of having those returned GIs to teach who were so tremendously eager to learn, you know, to work so hard. I've never seen anything like it before or since, certainly not since.

**Sopka:**

The classes were certainly large.

**Purcell:**

Yeah.

**Sopka:**

You know, I was in your Physics 28 class — in 1947-48.

**Purcell:**

Right, right. Well you remember those people. They were just fantastic. I mean, they would work like, you know, there was just nothing you could give them they wouldn't do, and — Yeah. Well the vacuum tube was still king. Chaffee's book was still full of important material. Saunders retired during the war — isn't that right? — sometime. He left.

**Sopka:**

Saunders I believe retired in '41 —

**Purcell:**

Maybe it was '41, something like that.

**Sopka:**

Kemble became chairman.

**Purcell:**

Kemble became chairman. That's right.

**Sopka:**

Apparently there was a change at that time. Saunders had been chairman for 15 years, so since that the chairmanship has been a short term duty.

**Purcell:**

Yeah. Well Saunders of course was a wonderful person to have as chairman for a young graduate student. I remember him with great affection, and he was awfully nice to us. My wife and I were married in '37. I didn't even have my Ph.D. yet. I was just a graduate student. You know, doing half time as a

teaching fellow, and Saunders helped us find a place to live and then in the summer of '37 they let us live in their house over in Berkeley Place all summer while they went to Ireland. And oh, it was fantastic. We have never reached that level of living in our life since. We had a gardener that came around, took care of the grounds, and a lady that came in and cleaned, and all we had to do was to keep the bird baths full. You remember he was a very serious bird person.

**Sopka:**

I didn't know that.

**Purcell:**

Yes. They banded birds, he and his wife. It was one of his many hobbies. Of course music, as we know, is his very important hobby, and he had started working on violins already.

**Sopka:**

Yes, in that period.

**Purcell:**

In the late '30s. The late '30s he was doing violin stuff. Right. He had his spectrum analyzer, his acoustic spectrum analyzer down here in the foot of the stairs here, in Cruft in that room. And if you go clear down to the basement and just inside that door was his thing. On the same level as the Mark I.

**Sopka:**

I see.

**Purcell:**

Of course prior to the Mark I there was that famous battery in there. Did anybody put that into your notes?

**Sopka:**

No, no.

**Purcell:**

The 100,000-volt storage battery?

**Sopka:**

I had read that there was one, but I haven't talked with anyone who ever saw it.

**Purcell:**

Oh, that was a fantastic thing. That filled the room that was later occupied by the Mark I computer down in the sub-basement of Lyman there on the east side. The money for it had been given to Professor Duane who did that famous work on determining the short wavelength limit of the X-ray spectrum, and he was to use that for his X-ray tubes so he'd have a very constant high voltage source. And this was built — I don't know, it was here already when I came as a graduate student, but it couldn't have been more than a few years old then. Duane died — when was that?

**Sopka:**

He died about 1938, but his work —

**Purcell:**

He was retired, because I don't ever remember seeing Duane as a graduate student.

**Sopka:**

He was in poor health —

**Purcell:**

Yes.

**Sopka:**

— from about 1930 on.

**Purcell:**

Yes.

**Sopka:**

His Duane-Hunt Law was done back in 1916 —

**Purcell:**

Yes.

**Sopka:**

— so that this battery that you're speaking about, was this something that had been in existence before the building of the Lyman Laboratory?

**Purcell:**

I don't think so. I don't see how it could have been, it was so big.

**Sopka:**

I see.

**Purcell:**

And it looked very new, and it was very elegantly arranged in stacks like book stacks in a library. You know, the whole room was full of these stacks. I mean, that's 50,000 lead-acid cells it was. Each cell was perhaps a little glass bottle that was 2 or 3 inches in diameter and perhaps 7 inches high. And there were 50,000 of those things all strung along very neatly and connected up with bus bars in different ways so it could all be connected in series or two halves in parallel. And we had a fellow named Charlie Lanza who was a lab assistant technician, also had a brother Sam Lanza. I think it was Charlie Lanza who one of his main jobs was nursing that battery, because you know he'd go around and put water in, and it takes quite a lot of time to keep 50,000 cells. But by the time I was a

graduate student, at least by the time I was doing my thesis, the battery had suffered a certain amount of depredation because people had gone in from time to time and walked off with a few thousand volts' worth.

**Sopka:**

Oh.

**Purcell:**

Because that was, you see, just before the days of voltage vacuum tube regulated power supplies, and if you wanted a really steady DC source in, shall we say 1936, '35, there was nothing better than several hundred 2 volt batteries strung together.

**Sopka:**

That's related then to the Byerly battery.

**Purcell:**

Yes. Well, we had battery sources for the laboratories for DC, but I think probably the Byerly thing was just a 50 volt battery or something like that probably, or it wasn't a very high voltage battery.

**Sopka:**

It was higher than 50 voltage, 50 volts. I'll have to see if I can determine how high.

**Purcell:**

Well, of course there is the thing that I mentioned to you some time ago that I learned in my astonishment that in — when was it? — in 1900 in Jefferson there was a 20,000 volt battery that was done, that was used by that fella who was repeating the Roland experiment.

**Sopka:**

Edwin Adams.

**Purcell:**

Edwin Adams, yeah.

**Sopka:**

Who ended up at Princeton.

**Purcell:**

Yes, that's right, and according to his account he used in his experiment a 20,000 volt battery that

Professor Trowbridge had apparently built over in Jefferson, which is no small piece of equipment.

**Sopka:**

No.

**Purcell:**

Well what happened to this one was that in about, it must have been about 1936, it was suddenly apparent that the regulated power supply would be the wave of the future and in fact the definitive paper on it was written by Hunt and Hickman, a very important reference. Roger Hickman and Ted Hunt wrote a paper on vacuum tube regulation of voltage sources or something like that, which was the basic bible for building and, theory of regulated power supplies. And it became clear that if you ever needed 100,000 volts DC very accurate, that the thing to do was to make a regulated power supply, that it would cost less than what we were paying Charlie Lanza to take care of the battery. So although the thing was in perfect working condition, it was clear that it was uneconomical to keep it. And I remember seeing at that time trucks pulling up right out here, outside the

corner of Lyman, and just shoveling in these hundreds of bottles and the truck going off piled —

**Sopka:**

It was just junked.

**Purcell:**

Just junked, piled with a ton of little glass bottles. They junked the whole thing — which is the only rational decision at that point.

**Sopka:**

Yes.

**Purcell:**

No one was actually using it at the time, but if they had wanted to use it, it would be crazy to do it, make a power supply. So they junked it, and then of course then that emptied out. I'm sure that one of the arguments was that if we junk it we'll have that enormous room then free for other things. And shortly thereafter the Mark I moved in — Whether Mark I was already in the wind when the battery was junked I'm not sure. My impression is that the battery junking occurred before Mark I, which after

all; Aiken didn't have the idea for that until '39 but —

**Sopka:**

I believe so. I was told that the Mark I was actually constructed at the IBM laboratories in Endicott and was shipped when completed to the Harvard basement.

**Purcell:**

Yeah, okay. Well then the junking of the battery occurred before the — I don't know what was in the room then between, after the battery was — That was a period when I was then an instructor. I'd gotten Ph.D. in '38, and '38-'39 I was an instructor and Jack Livingood and I shared an office up in Jefferson while we were both working on the cyclotron. The teaching I remember from that — Well, my first teaching at Harvard I think was under N. H. Black.

**Sopka:**

Yes, that's quite likely.

**Purcell:**

I was a teaching fellow in Physics B.

**Sopka:**

Yes, that was his course.

**Purcell:**

Which he ran in his very well planned and organized way. So that was when I was — I guess in '37 probably was my first teaching in that. I was a lab instructor. The laboratory was up where our tea room is now, you know, at the end of the building. That was the old Physics B lab.

**Sopka:**

I didn't know that. I took Physics B at Radcliffe where we also had Professor Black.

**Purcell:**

The room then became our undergraduate library — later.

**Sopka:**

Yes.

**Purcell:**

That was the Physics B lab. Yeah. Over at Radcliffe of course everything was given over there in double. And he ran this well organized course. He was actually — I have great respect for Black. He didn't know a great deal of physics certainly, and of course was not one that we would now hire — oh, it was pretty much practical physics, high school physics kind of. But it wasn't all that bad, and —

**Sopka:**

You may be amused to let me put in a little comment of my own about Black.

**Purcell:**

Yeah.

**Sopka:**

At the end of the year a classmate of mine and I decided that physics was for us, and we went up to tell him. We thought that he'd be pleased.

**Purcell:**

Yes.

**Sopka:**

That on the basis of his course two girls are going to physics. And he got the most peculiar expression on his face and said, "But physics is no place for a woman. Why don't you go and take chemistry or biology?" Well that particular reaction was just sufficient to confirm any doubts that I may have had in my mind physics was for me. [laughs]

**Purcell:**

Very good. No, I can imagine him saying that. He was very old fashioned of course in his views about things of that sort. He'd come, you know, from Roxbury Latin where he had been Conant's teacher.

**Sopka:**

Oh, no, I didn't know that. I knew he had taught, Black had taught in the high schools.

**Purcell:**

No, he was quite a well-known secondary school teacher, and as I say, young Conant had been in his class, Roxbury Latin I'm pretty sure it was. And Conant brought him to Harvard.

**Sopka:**

Oh.

**Purcell:**

One of the early, first things Conant did as president, brought Black over into the School of Education.

**Sopka:**

I see.

**Purcell:**

And Black was supposed to be the School of Education teacher. And Black didn't like it in the School of Education. To his credit he found that what was going on there wasn't — So he came over and taught the elementary courses in physics. And he remained an assistant professor, you know, his whole life.

**Sopka:**

Yes, I knew that he never did any research.

**Purcell:**

He never did any research, never pretended to. As I say, I had great respect for him, because for all his limitations he was a very straightforward, modest, you know, and a great, great character.

**Sopka:**

And certainly for physics at that level, he was a very effective teacher.

**Purcell:**

Very effective, yeah, yeah. It was a well-organized course. The students knew always, as you remember yourself, what he —

**Sopka:**

And he had at least two books that he had written.

**Purcell:**

Oh, he had innumerable books. You know his books — I studied high school physics from his book, Black and Davis, which was the high school physics text of the 1920s. There's no question about it. And then he had a chemistry book that he wrote with

Conant. I'm sure Conant wrote most of the book, but — and Harvey Davis, who was the Davis of Black and Davis, was at Harvard then, Dean of Engineering or something. But Black's books sold more than 2 million copies total I learned one time.

**Sopka:**

Oh. Because then he had also an elementary physics book that was just under his own name.

**Purcell:**

Yes. Practical Physics it is called I think, something like that. That's the book we used in Physics B.

**Sopka:**

Yes.

**Purcell:**

That's right. This high school book was Black and Davis, and Black and Conant was the chemistry text. They were sort of secondary school level books, even for those days, but they were practical. You learned how an automobile works, and things like that.

**Sopka:**

Sure.

**Purcell:**

And then I worked under Saunders in all of Physics C, which was I guess you know [???].

**Sopka:**

Physics C was, according to the catalog, only open to students who had already taken physics in high school.

**Purcell:**

Yes, yes. Physics C was the present equivalent of Physics 12.

**Sopka:**

Yes.

**Purcell:**

And Physics B was the present equivalent — was equivalent to the present Physics I, a very parallel thing. Physics B was the thing that premeds took and people that hadn't had physics before and weren't

going to take it again and had to take it for one reason or another. They were in Physics B. But Physics C was Saunders, and, that was quite a education for a future physics teacher because Saunders was a person who talked with great — or very debonair and charming figure. And I remember his famous lectures in acoustics in Physics C, you know, when he would play instruments and stuff like that were so — One time he — if you remember the demonstration called the "Chladni Plates"?

**Sopka:**

Yes.

**Purcell:**

You know, where you have those —

**Sopka:**

The patterns that sound —

**Purcell:**

Patterns. Yeah, yeah, you have a square plate and a round plate and a triangular plate and then you sprinkle sand on 'em and stroke them with bow —

**Sopka:**

Yes.

**Purcell:**

And then you get these patterns.

**Sopka:**

Patterns.

**Purcell:**

Well I'd seen Saunders do that, and it looked absolutely easy to do. There's just nothing to it, you know. And that semester, one semester he was going to be away, had to go to a meeting or something, and he asked me to give the lecture, that particular lecture. So I did practice the day before. I went over there and got the Chladni plates out and got the bow, put the sand on the plates, and I couldn't make the patterns come out at all. You know, and it was a mess! And the more I tried, the worse it got. And I tried it in lecture and it didn't work either. And of course but Saunders was, you know, he was a musician, played the viola, and when he took a bow in his hand —

**Sopka:**

He knew what to do with it.

**Purcell:**

— he knew what to do with it, whereas — [laughs] Oh dear. And all the other things. Blowing — I think you had to blow a clarinet or something and it was a real fiasco. Well, and of course Lyman was around. When I was a graduate student I listened to Lyman's lectures in optics, physical optics.

**Sopka:**

Oh.

**Purcell:**

And then Lyman was a figure there, always in the colloquium, he'd be sitting there, front row. Of course Lyman would have given you the same advice that Black did probably, about majoring in physics.

**Sopka:**

That is very likely, because I found a letter that Bergen Davis wrote to Lyman about a girl who had

taken her degree with Bergen Davis, her undergraduate degree, and was looking for graduate work, and she was from the Boston area. And Bergen Davis wrote to Lyman and suggested that she do graduate work at Harvard. Lyman's answer, the gist of it was that he didn't know how to cope with a woman at Harvard [laughs].

**Purcell:**

That's right, that's right.

**Sopka:**

They'd never had one, and he didn't know what he'd do with her if she came, and couldn't she go someplace else!

**Purcell:**

I was thinking of Lyman one time years ago and I was colloquium or seminar or one of our very bright physicists was sitting there nursing her baby, you know, calmly, but I was thinking of Lyman in there turning around and seeing that one [laughs]. He was a very dignified gentleman.

**Sopka:**

Apparently Lyman never married.

**Purcell:**

Oh no, no, he never married. No, oh, no. He was — As was Professor Hall.

**Sopka:**

Oh, I was never aware of that.

**Purcell:**

Professor Hall was an interesting figure too in those years, in my years as a graduate student.

**Sopka:**

He was still around, but he had retired.

**Purcell:**

Oh no. You see, he came in to the laboratory every day and he did experiments on the Hall Effect.

**Sopka:**

I didn't realize that he was still active in the '30s.

**Purcell:**

Oh yes, yes. He didn't produce anything much, but he had the room, which is now I guess Arthur Jaffe's office right up here.

**Sopka:**

Yes?

**Purcell:**

Although it's a smaller room now. And in this room he had a magnet and he did research on the various effects, you know, of the Hall Effect, etc. in that magnet.

**Sopka:**

I see.

**Purcell:**

And he — by then he was quite old. When I was a graduate student he was really quite old. His hands shook quite a bit. But he had the magnet fixed up

with sort of wooden guides so that when he got his little sample together and everything, if he could get it started in the guide he could push it into the magnet alright.

**Sopka:**

Oh. Good for him.

**Purcell:**

And he had thermocouples and galvanometers and he worked in there all by himself doing this. And he came to the laboratory every day, and in the afternoon he'd go into the research library and take a nap, sitting back in his chair in the corner, take a nap. And you know, since I'm approaching that age myself, I say it's a very, very good thing to do. In fact I remember a story about him that he was a man with a — very unobtrusive, didn't say very much, but he had a kind of a dry sense of humor whenever he did talk. I had a colleague, I had a friend who was in graduate school when I was a graduate student, a very brash fellow, really — those days, then nowadays — But he was fascinated by Professor Hall and one day he said to Professor Hall something like this: "Professor Hall, it seems odd

that after all the distinguished work you've done you wouldn't sort of take it easy, that you still come to the laboratory every day and work," you know, something like that. To which Hall just looked at him with a sort of twinkle in his eye and replied, he said, "Well," he said, "you see I have to be near a toilet." [laughter]

**Sopka:**

Like a very earthy gentleman.

**Purcell:**

He put him down [laughs].

**Sopka:**

I've been impressed with the longevity of the Harvard physics faculty.

**Purcell:**

Oh yes, my goodness.

**Sopka:**

And not only their physical, but their research longevity.

**Purcell:**

Yes. I remember a terrible crisis at that time when somebody — Hall had made his thermocouples out of a particular spool of manganin and advance wire, whatever combination he used, in the stockroom where they had, you know, spools of different — And he did that for the very good reason that you didn't want to recalibrate every time. You want to use the same alloy.

**Sopka:**

Yes.

**Purcell:**

And some graduate student had checked out that spool of wire from the stockroom and it was missing. Sparks were really flying for a while. I don't know whether he got it back or not. No, Hall was an odd fellow. See, in a certain sense he never understood the theory of the Hall Effect, because the Hall Effect was only really explained by quantum mechanics.

**Sopka:**

Yes. That's right. So it would come too late for him.

**Purcell:**

And he had a theory, he had developed a theory of his own which he called a dual theory of conduction. He had a little book on it. It probably should be still in the library — a dual theory of conduction.

**Sopka:**

Oh. I'll have to look that up.

**Purcell:**

Yeah. And I remember one time when, it must have been — I can date it almost, it must have been '38, '37 perhaps. When R. H. Fowler was here he gave a colloquium — you know, the great British theoretical physicist, R. H. Fowler. And Fowler was talking about something having to do with the electron theory of metals. I forget the exact topic. And Professor Hall was sitting in the front row where he always sat, and at the end of the talk, the question period, Professor Hall mumbled some question that he didn't, you know, showing his

general lack of enthusiasm for what was going on, to which Fowler may have said the most graceful reply. He said, "Well Professor Hall," he said, "I should think you would welcome this theory because this is the first theory that explains the positive Hall coefficients." [laughs] Of course Saunders kept on teaching for many years after he left Harvard, out at Mount Holyoke.

**Sopka:**

Oh, I wasn't aware of that.

**Purcell:**

Oh yes. Saunders retired to Mount Holyoke.

**Sopka:**

I see.

**Purcell:**

And he and his wife built a house at South Hadley. I think maybe she had had some association there. I don't know, they'd had some association somehow with Mount Holyoke before, and Saunders taught there for many, many years.

**Sopka:**

Oh.

**Purcell:**

We had been out to visit them back in those days.

**Sopka:**

I believe Saunders lived until 1960 or '61.

**Purcell:**

Yes, and of course she just died a couple years ago.

**Sopka:**

I see.

**Purcell:**

And there he continued his violin work there, very seriously. So he counts in the longevity list too.

**Sopka:**

The nature of physics certainly has changed in terms of what things one does if one is a physicist within the Harvard Physics Department.

**Purcell:**

Well, yeah, the tools are of course different so that the experimental work one now uses much more powerful aids, so that every experiment is festooned with things we didn't have — online computers and vacuum pumps unlike the old ones, and stuff like that. But I don't see that, I don't feel that is any real change in kind.

**Sopka:**

No, but doesn't it appear that many of the things that would not be done within the physics department, for instance if one wanted to study violins or trumpets would that be done within a physics department? Where would you do that?

**Purcell:**

Well, it is being done in some physics departments. I mean, there aren't many people doing that, because I don't think there's all that much to do in that, but there are two or three people like Arthur Benade at Case, whose career has been the physics of instruments.

**Sopka:**

Oh yes. I remember his book.

**Purcell:**

And that's — it's not a thing we'd ever — In fact, Saunders didn't consider that he was doing, that it was — It was sort of his end of career hobby so to speak I think even when it began right here in the department. Although it was interesting at the time, because he was well in advance of others in looking at the acoustic spectrum environments to find that new technique, then new technique, to the problem. Well, I don't know. If you — the high-energy work of course has developed a very different style because the one that perhaps could have been predicted already in the '30s, because now the machines are so big that they can't be at the universities, and so people have to do their work by these enormous cooperations at the big laboratories. Whereas in the '30s and immediate postwar years, if you were a physics department you had your own cyclotron and I think physics came out of the Berkeley cyclotron or the Chicago cyclotron, you know, or the —

**Sopka:**

Did you have anything to do with the construction of the postwar cyclotron at Harvard?

**Purcell:**

No, not a thing.

**Sopka:**

I see.

**Purcell:**

By then I was completely absorbed in the nuclear magnetic resonance stuff. I was aware of the planning and heard people discuss it, but I hadn't —

**Sopka:**

You didn't have any responsibility.

**Purcell:**

I had no responsibility and no input to it at all.

**Sopka:**

I see.

**Purcell:**

Theoretical physics of course is — I suppose one might say there's a change in style there too, but only in the scope of the enormous number of people working and it —

**Sopka:**

The acceptance of the importance of the theoretical activity on the part of physicists seems to have changed over the years. David Webster got his degree in 1913, and for an experimental thesis it apparently was very difficult for him.

**Purcell:**

Yeah.

**Sopka:**

He told me that he was told that Harvard would never give a degree which was not based on some experimental work.

**Purcell:**

Right. Yeah, that's certainly changed here in this department. I guess Kemble was the first one to get such a degree, wasn't he?

**Sopka:**

Yes, but even Kemble's thesis was part experimental.

**Purcell:**

Uh-huh [affirmative].

**Sopka:**

And Van Vleck was the first quantum physics —

**Purcell:**

First purely theoretical.

**Sopka:**

— purely theoretical quantum physics.

**Purcell:**

Slater's thesis was experimental.

**Sopka:**

Experimental. Yes.

**Purcell:**

Yeah, yeah, right.

**Sopka:**

On the other hand I, in reading the annual reports, I found that by the end of the '50s the department was concerned that there were too many theoretical students in proportion —

**Purcell:**

Yes. Oh yes. No, that's right. Everybody had — Well, I think it seems to me that experimental physics has really gotten quite difficult in that if I were, you know, I started a young graduate student or young postdoc in experimental physics, it's a rather forbidding situation in that it's hard to think of anything you can do that doesn't involve so much of an investment in equipment that the only way to do it is to join an ongoing research operation. You can't just go off yourself and have an idea and go off and do it, because you can't afford to buy the stuff it

would take to do it, and furthermore there are so many people hacking away at the business it's kind of hard to scratch up something that somebody isn't already doing.

**Sopka:**

Yes.

**Purcell:**

Whereas in my time as a graduate student, looking back on it, the difficulty in finding something to do that nobody was doing wasn't serious at all. I mean, almost any reasonably good idea would have a chance of being new. You know, it's so that the individual experimenter who is able to have an idea himself and go and try out that wonderful experience is pretty hard to have, you know.

**Sopka:**

Does the graduate student today who wants to be an experimental physicist include in his training shop techniques such as were done 50 years ago when graduate students had to learn how to blow glass and operate lathes, a standard part of their training?

**Purcell:**

Well, I don't know. I'm not a very good person to ask, because I haven't had experimental graduate students for a long time, but my impression is that they have to know some electronics.

**Sopka:**

I see.

**Purcell:**

I suppose that for many of them, for some of them here, certainly they have to get some shop experience or pick it up one way or another, but many of them don't. Glass blowing of course is totally obsolete.

**Sopka:**

Yes.

**Purcell:**

But I believe that for the typical present day experimental graduate student the thing that he's absolutely got to have is familiarity with modern electronics.

**Sopka:**

Including computer science?

**Purcell:**

Oh yes, including, yes, yes, include that in. In fact he's got to — well, what he needs to know is most accurately defined by the content of Paul Horowitz's course 123.

**Sopka:**

Oh, that's interesting to check out there.

**Purcell:**

That is exactly what, you know, if you had any one thing that a present experimentalist needs, it's that.

**Sopka:**

It's in that course.

**Purcell:**

Now there are all these other things that he has to have some lower on depending on his field, like high vacuum stuff. See, one of the changes between my period as an experimentalist and now is just in the

degree of vacuum that you can get. People don't realize how much it's changed.

**Purcell:**

By five orders of magnitude. If you really want a good vacuum now.

**Sopka:**

Well, I wasn't aware of that.

**Purcell:**

Yeah. It used to be, you know, 10 to the minus 5. We'd get down to 10 to the minus 5 and the —

**Sopka:**

That's pretty good.

**Purcell:**

Yeah, yeah, yeah, it's pretty good. You know, the old pump would be going there clack-clack instead of glug-glug, and the ionization gauge would be reading 10 to the minus 5, well you were home free.

**Sopka:**

Sure.

**Purcell:**

In a vac'd out small system now they think nothing of 10 to the minus 10.

**Sopka:**

Oh. That's —

**Purcell:**

And they'll go 10 to the minus 7 in a great big accelerator, and the pumping is an entirely different business, those titanium pumps.

**Sopka:**

Oh. Yes.

**Purcell:**

No mercury around the place. A modern laboratory is distinguished by the total absence of mercury in any form. I guess a little bit into fluorescent lights, but otherwise —

**Sopka:**

And none rolling around the floor like they used to —

**Purcell:**

It's not rolling around the floor.

**Sopka:**

— warn us about.

**Purcell:**

And boiling in the pumps. God. And those traps we used to have to keep closed. Well that and of course then the whole rebirth in optics is one of the great things, on account of lasers and everything, that whole physics of, the experimental physics of light is just a totally new subject now, absolutely remarkable.

**Sopka:**

Oh yes. Would you be inclined to comment on the events surrounding your being awarded the Nobel Prize? You've mentioned the kind of work, your nuclear magnetic resonance research.

**Purcell:**

Well, I don't know. I think the more interesting comments are perhaps on the experiment itself and not the Nobel Prize.

**Sopka:**

They're in your lecture, your Nobel lecture presumably.

**Purcell:**

Yeah. But our relationship for instance to the Stanford work is sort of interesting, and I'd like to say a word about that just to have it on record.

**Sopka:**

That would be fine.

**Purcell:**

We began thinking about the experiments. I really had the idea for the experiment in what I think was August 1945, and enlisted the help of Henry Torrey and Bob Pound. Bob, because he was, I knew him, he was a good friend, very young then, but he was of all the people at the Radiation Lab, Bob was the

sharpest on questions of signal-to-noise receivers and stuff, and Henry was mostly involved in that too. Henry Torrey moreover had been a graduate student at Columbia in the molecular beam, at that time the beam stuff, and knew that side of it, and furthermore was a very competent theorist. So we made a good team in that respect, and we couldn't have done it I think — See, every person was indispensable. We couldn't have done it without Bob, couldn't have done it without him. And we had no knowledge whatever of Bloch's idea and what he was working on at Stanford. Totally. Although Felix I think had been thinking about it maybe already before he left Cambridge to go back to Stanford, but at any rate I didn't even know him then. Knew who he was and seen him, but I — So the only thing that gave us pause in the course of working on the experiment, and it occurred after we'd I guess even after we'd tried it once and failed, that we learned that two or three years before Gorter at Leyden had had essentially the similar idea and tried it and it failed. We learned this because we got a photostat or something of the article which hadn't been coming over during the war, but somehow I came over and found it here and read it. It wasn't a photostat, you

know, it was a what do you call it? Microfilm I guess. And some — I don't know who put us onto this or who heard this rumor, but we looked it up and sure enough Gorter had tried what ought to have worked to measure nuclear resonance, with crystal of — I forget what the crystal was at the moment. Well at any rate, we decided we knew why it failed, because it was the thing that we had already been taking into account and preparing for connected with the relaxation time. We thought he had saturated this stuff before the resonance, so we decided to go ahead and try again, and worked on it, and finally we made it work — all of this before we even knew what was going on at Stanford. And then we heard about that, and they, after they were working on it, and they finally got the resonance about a month or so later, Stanford, using Bloch's technique of the crossed coils. The first actual communication we had with that group was with Bill Hansen, a very interesting man who collaborated with Bloch on this experiment but who was very well known to the rest of us as one of the inventors at Klystron and had managed to spend some time at Radiation Lab lecturing on microwaves, and we knew it would be a tremendous expert and very fun fella, but Bill

Hansen came east I guess it must have been perhaps March of '46 and we started explaining our mutual experiments to one another. You know, it was clear that they were basically getting the same result, but we had approached it from such a different direction that literally we talked with Bill for about a half an hour before we established where we could each see it from the other's point of view.

**Sopka:**

Well, that's very interesting. I wasn't aware of that.

**Purcell:**

Yeah. They had always thought about it, in a perfectly correct way, in terms of the precession of the expectation value of the moment so to speak, so they had just thought about it classically and the precessing moment, which was the basis for the two-coil method, one coil drives and the other coil picks it up. And we had thought of it in terms of inducing transitions between two energy levels purely quantum mechanically and thought of the absorption of energy from that, and we had detected it therefore not by looking at the precessing out of phase

component but by detecting the absorption of energy to resonant cavity.

**Sopka:**

I see.

**Purcell:**

Now of course these are actually identical, of course identical, things, and in class if I were lecturing on it I would do it both ways, but we looked at it in such a different way that — Then among the very important things that happened here in '46, I ought to mention, was that Bloembergen arrived, and Bloembergen turned up one day, and it must have been — oh, it must have been, I don't know, again April, perhaps April '46, something like that, here turned up this young man who had just gotten his AB at Utrect and who came without any advance notice or introduction or anything. Nobody knew who he was; he just showed up here one morning and said he had gotten his Bachelor’s degree in Holland and he was interested in physics and wondered if there was any job for a laboratory assistant. And I talked with him for a while, and we had not thought we needed a laboratory assistant, and the thought hadn't occurred

to us. We didn't have much money to pay anybody. But after I talked to him I decided it would be a good idea to. It was the smartest thing I ever did. So we hired Nico as laboratory assistant. He was practically fresh off the boat from Holland. You know, and within a couple of weeks he was writing, he was ordering things, building stuff, doing experiments, you know, really going.

**Sopka:**

Good.

**Purcell:**

And it was therefore the work that he did then the next year that was really the basis for all the work on nuclear relaxation. He went back to Leyden then and got his Ph.D. at Leyden but with a thesis that he did here. So in a very real sense my first graduate student got his Ph.D. at Leyden with Gorter, and amusingly, while he was there he got the crystal that Gorter had actually tried the experiment with and got the resonance in it.

**Purcell:**

So it really was saturated.

**Sopka:**

Oh. That makes a nice fulfilling of the original idea.

**Purcell:**

Yeah. It's really too bad for Gorter, because Gorter in a way almost you could say deserved to do the experiment first, because he was one of the first to point out the importance of nuclear magnetism and things like that, so that it was a case where Gorter — well, the only way to say it is Gorter was unlucky and we were lucky in that respect. There is no other way to describe it. Bloembergen then gave — we published then. Certainly the paper I had been associated with had more references than any other; it must be by far, an enormous margin. This was a paper that came to be known BPP, published in '46, which was our first big paper on the relaxation stuff, Bloembergen, Purcell, and Pound, and was really Nico's thesis sort of turned in — I wrote the paper, but while he was writing his thesis, and it was sort of published.

**Sopka:**

That's very nice to get that story down. We're coming to the end of this side of the tape.

**Purcell:**

Oh, alright.

**Sopka:**

So that I don't think that we want to start a new topic.

**Purcell:**

Have you ever seen the pictures that we have of those old people? Did I show you those? You know the stereo pictures we have? Do you know about those?

**Sopka:**

No. Maybe I can turn this off anyway now.

# Interview Session – 2

**Sopka:**

Professor Purcell, I'd like to begin by asking you to tell me something about your early childhood. I understand you were born in Taylorville, Illinois, but I must confess I don't know exactly where that is.

**Purcell:**

I grew up in two small towns in Illinois. Taylorville was the first one, and it's down about in the middle of the state a little bit south of Decatur. When I was about 15, I guess it was, we moved to Mattoon, which is a town some 60 miles to the east, south of Urbana. Those towns were my home until I was through college and moved away. I had a childhood that was typical of those times and that place, a very happy childhood as I look back on it. My father was a businessman, manager of the local telephone company. This was an independent telephone company, not part of the Bell system. It survives today, as do many of those small, independent companies. He was manager of the telephone exchange in Taylorville and then later in Mattoon

was general manager of the Illinois Southeastern Telephone Company, which had the telephone business in three or four counties in that part of Illinois, a handful of towns with a population of a few thousand to 10,000 each. My father had grown up on a farm near Taylorville and his own education was extremely limited, although it included a spell in teaching a one-room country school, so that he had an interest in and a respect for teaching. My mother, on the other hand, had been rather highly educated. She was a graduate of Vassar and indeed had an M.A. from Vassar in classics. She'd grown up in Decatur, which was always for me the big city of my childhood, and my mother had been a high school teacher in Taylorville, teaching Latin, at the time she and my father were married around 1910.

**Sopka:**

Were you their first child?

**Purcell:**

Yes, I was the first child, and I have one brother five years younger also born in Taylorville before we moved away. I had many childhood friends in our neighborhood. It was a rather homogeneous

neighborhood. Taylorville itself was a farming town in the middle of a farming community, but also a coal mining town. There were extensive soft coal mines in that vicinity, still busy I believe; and we were familiar with all the sociology of a small mining town, including, as I remember, the early years of the growth of the unions in the mines. I had no interest that you would identify as scientific when I was in Taylorville, being young. However, the fact that my father was working for the telephone company had already begun to have some influence. The telephone office, as we called it, was a place that had a back room where all the switchboards and technical equipment was. In the basement there were discarded sections of cable and wire, and I could bring home items like that from the telephone office. It was my source of wire and of lead, because if you got an old hunk of telephone cable, you could melt the lead sheath and then take the paper insulation off the wire, and you had the makings for a lot of things. We moved to Mattoon, which is a town of about the same size, perhaps a little bigger than Taylorville. I had my last two years of high school in Mattoon. There I had a particular close friend, a boy of my own age, who was interested in chemistry and did a

lot of home chemistry experimenting. And I think it was our joint activity and friendship then that probably further stimulated my interest in some kind of science or engineering.

**Sopka:**

Did you read much as a child? Your mother's background might have encouraged you to.

**Purcell:**

Yes. Ours was a house that had many books in it and I did read as a child. My reading wasn't vast, but …

**Sopka:**

At least a suitable activity for a summer afternoon.

**Purcell:**

Yes, literature and books — children's books and children's classics and so on — were a familiar part of my childhood, and also I spent a good deal of time in the public library.

**Sopka:**

Were the churches of these towns — Taylorville and Mattoon -of considerable social influence in the communities?

**Purcell:**

Yes, yes. (I should have mentioned that.) My mother and father were Presbyterians, and I attended Sunday School at the Presbyterian Church and indeed spent a good deal of time singing in a boys' choir in the Presbyterian Church, which is probably where I heard more sermons than I've heard in total since. It was a town with a number of churches. A town of 10,000 in rural Illinois at those times would have a half a dozen churches of all the well-known denominations. I don't remember any great intellectual concern for religious questions until we moved to Mattoon, where, as it happened, this friend of mine, whose name was Dunlap McNair, whom I have mentioned, attended … Well, his father and his uncle were rather strong-minded, church-oriented people. Indeed his uncle was ordained, although he didn't have a pulpit. Dunlap and I found ourselves in sharp conflict with the theological views of his

father and uncle, and we began reading some Unitarian tracts which greatly attracted us. In fact, I think if there had been a Unitarian church in Mattoon, we might have found a place we could go. As it was, we simply had arguments which I left us disgusted with the whole proposition. And I guess although I'm perhaps still carried on the rolls of the Presbyterian Church in Mattoon, I really have not considered myself an active church member since my bitter conflicts, intellectual conflicts, with Uncle Irving.

**Sopka:**

The school that you went to in Taylorville, was that a regular eighth grade set-up where you then moved on into a four-year high school?

**Purcell:**

Yes, actually in Taylorville we had six plus two plus four. I moved after the sixth grade into what we called a junior high, but it was essentially the same as an eight-year elementary school and then four-year high school.

**Sopka:**

Was there any difference in the offerings between what you would have had available if you'd stayed in Taylorville in contrast to going to Mattoon?

**Purcell:**

No, I think there was no difference of any consequence. In Mattoon I had high school chemistry and high school physics, both of which were rather important to me or seem so in retrospect. The chemistry teacher, a man, was very good; and really it was the first time I'd encountered any grownup who was a real scientist, if I can use that term. Mr. Thomkins really knew some chemistry. He was seriously interested in what he was teaching. In fact, it was a better chemistry course in some ways than my subsequent chemistry course at Purdue. In physics the teacher was a woman, Miss Edwards...

**Sopka:**

That's surprising in a sense — although maybe not.

**Purcell:**

…who didn't know a great deal of physics but… She was a person who understood her own limitations and respected the subject and was utterly honest and sincere, so that in fact I think she introduced me to physics in a humane way that probably was important. One anecdote of those times that I sometimes tell in her memory was: we were using Black and Davis's text, which was widely used in high school physics in those days. Much later in my life I worked as a teaching assistant under N.H. Black, but in those days Black and Davis was the standard high school physics text. It had the famous problem of the man who pulls himself up the flagpole by sitting on a seat from which a rope runs up and over a pulley at the top of the pole and back down. He pulls on the rope and up he goes. And, of course, the question is: how hard does he have to pull? And Dunlap, my friend, and I thought we had figured out the answer and it was that he would have to pull with half his weight. But Miss Edwards said, "No, it says in the book that a fixed pulley has no mechanical advantage, and there's no question that this pulley was fixed at the top of the mast. So the answer must be that he pulls with his whole weight."

Well, we couldn't accept this, and as it happened, we had a barn and Dunlap had a scale for weighing ice which would weigh up to 100 or 150 pounds — I forget which.

So we went into the barn after school and rigged this thing up with a seat and hooked the spring scales to the upgoing rope and then pulled on the downcoming rope. I still remember exactly how much I weighed at the time, because I started off sitting on the seat, and when the scales read 60 pounds I started to go up; I weighed 120. And then Dunlap got in and tried it and we ran all the way to high school to announce our triumph, and when we got there Miss Edwards was still up in her room grading papers, where you were likely to find a conscientious teacher in those days. And so we told her all about it. Her response was instantly: "Well, I must have been wrong and you must be right because you did an experiment and proved it," which I thought was a gallant response and exactly the right one. I've always felt that she did me a real service at that moment. Well, at any rate, Dunlap was the avid chemical experimenter. We tried some other things. I remember we tried making a galvanometer and a few things. They were not

particularly successful. We tried a Tesla Coil. I think it worked. Again, I had a source of some material at the telephone company. You could always get plenty of the bell-ringing generators that were in the old telephones, which consisted of a series of horseshoe magnets making the stator field and an armature that was wound with what must have been a mile of number 39 wire or something like that.

These made good shocking machines if nothing else. I also did a few odd jobs at the telephone company, including helping to make maps of their distribution of lines in the city, and things like that of kind of engineering nature. One of the important contributions that came to me by way of the telephone company at that time was the BELL SYSTEM TECHNICAL JOURNAL. Although the Illinois Southeastern was not part of the Bell system, they nevertheless did all of the maintenance on the AT & T lines that went through their area, and they regularly received the BELL SYSTEM TECHNICAL JOURNAL. It was not avidly read in that company. There were practical wire men and so on, but there were no people who were interested in articles in the BELL JOURNAL. So I could take those things home and read them. They were

fascinating because for the first time I saw technical articles obviously elegantly edited and prepared and illustrated, full of mathematics that was well beyond my understanding. It was a glimpse into some kind of wonderful world where electricity and mathematics and engineering and nice diagrams all came together. Just a little later (I'm not quite sure of the dates of this) the BELL SYSTEM JOURNAL began publishing Karl Darrow’s articles on modern physics.

**Sopka:**

They, I believe, would have begun while you were still in high school.

**Purcell:**

Well, then I began reading them. That is, I don't think I appreciated them so much until I had been in college and came back to them, since we had them, you know — I kept them.

**Sopka:**

You would have been in high school around 1926.

**Purcell:**

I graduated from high school in ‘29.

**Sopka:**

Karl Darrow was definitely publishing his articles by then.

**Purcell:**

Yes. I believe that… I think the first thing that really fascinated me about the Bell System's journal as a high school boy was not Darrow's articles yet, although they may have been there, but they didn't make the impression that they did later, but just the technical articles — I mean the wonderful, beautiful diagrams… The BELL SYSTEM JOURNAL was a beautifully edited journal then and it is now. You know, the diagrams were superb and everything was really just right. I had those copies of the Darrow articles for a long time. I had them when I was at Purdue.

**Sopka:**

While you were in high school did you go into extracurricular sports or drama or any such activity,

or were you pretty much oriented towards scientific things?

**Purcell:**

Well, we played tennis. I played tennis rather seriously in high school — not seriously in the modern way of having teams and so on, but my particular friends began playing tennis in the park. It had rather poor facilities. And we had swimming. I went to summer camp, a small YMCA summer camp, when I was in Taylorville and also in Mattoon. They were just the summer sports of a small town. We played baseball a good deal, didn't play much football. I was a Boy Scout (only in Taylorville) but never got to be more than a Tenderfoot because the main activity of our scout troop was playing basketball. We had a basketball team but we didn't ever do much scouting. That was a very nice time to be growing up in a small town in the Midwest.

**Sopka:**

By the time that you were ready to go to college, your decision to go to Purdue and major in electrical

engineering was not inconsistent with your high school experience.

**Purcell:**

No, it wasn't. It came naturally out of that. I don't remember anymore exactly why I didn't go to the University of Illinois, which being only 40 miles away was of course the great institution in our eyes. But it was the idea of being an engineer, and Purdue was smaller — of course much smaller than the University of Illinois then. When I went to Purdue it had only 4500 students. My friend Dunlap also wanted to be an engineer, a chemical engineer, and he went to a very tiny but a surprisingly good engineering school called Rose Polytechnic in Terre Haute, which is rather near Mattoon. It's just really across the border in Indiana.

**Sopka:**

But at the time you enrolled at Purdue, was it a general liberal arts type curriculum, or was it definitely oriented more towards engineering?

**Purcell:**

Well, Purdue was part of the state university system in Indiana, and it was the engineering and agriculture and home economics part of the state university, the other of which was the University of Indiana at Bloomington. So if you wanted to study liberal arts in Indiana, you'd go to Bloomington. Purdue was an engineering school: engineering, agriculture, and home ec for girls.

**Sopka:**

Were there any problems with regard to your paying tuition in Indiana that you might not have had to pay if you were in Illinois?

**Purcell:**

Well, it was very small. Goodness, I don't remember — maybe $50 a year that you had to pay. As an out-of-state student you had to pay a little more, but even in those days the difference was not very great. No, that would have mattered. I mean my family could not have afforded easily to send me to an expensive eastern school, for instance. That was not really considered. But I thought I wanted to be an

electrical engineer, and the idea of being a physicist at that point just wasn't an image that one had to consider somehow. You see, in the '20s the idea of chemistry as a science was extremely well publicized and popular, so the young scientist of shall we say 1928 — you'd think of him as a chemist holding up his test tube and sighting through it or something. And that was the result of the experience and history of World War I where the United States had to develop its chemical industry practically from scratch because German industry had previously supplied all that.

**Sopka:**

Did you by any chance during this period come in contact with the book by Slosson called CREATIVE CHEMISTRY?

**Purcell:**

Oh, yes. Everybody read that.

**Sopka:**

Several people have mentioned that as being very influential.

**Purcell:**

Oh, sure, we both read it; and, you see, my friend Dunlap was right on that groove. No, E.E. Slosson's CREATIVE CHEMISTRY — sure, we read that, and it was all very exciting. I don't know why I didn't want to be a chemist, looking back on it. I found the chemistry course at Purdue somewhat interesting. But there was no idea of what it would mean to be a physicist. The subject, I knew about — but being a physicist… I don't remember considering that at that time as something you could be.

**Sopka:**

The choice was electrical engineering or chemistry.

**Purcell:**

Yes, sort of like that, yes. In fact, I've sometimes remarked that to me at that time the name Steinmetz was more familiar and exciting than the name Einstein, because Steinmetz was the famous electrical engineer at General Electric and was this hunchback with a cigar who was said to know the four-place logarithm table by heart and all that kind of thing. So it was only gradually as a student at

Purdue that I began to learn not only what physics is but what it would be to be a physicist. So I went to Purdue in the fall of 1929.

**Sopka:**

That was the initial year of the Depression.

**Purcell:**

Yes, and you know, looking back and trying to remember my own view of my surroundings at that time, I really … the Depression was never really very much in the forefront of my consciousness.

**Sopka:**

Presumably your own father's job was stable.

**Purcell:**

Well, my father's job was stable — that's true — and he hadn't made any catastrophic investments or anything, but in those small towns in Illinois banks were failing all around. In Taylorville most of the banks in the town failed. It was really a tragic time for a lot of people even in that part of our society, and yet I was so interested in what was going on in

college and in learning things and all that that it just didn't make much of a dent on me.

**Sopka:**

Did you work while you were in college?

**Purcell:**

No, I had some odd summer jobs at the telephone company, things like that, but I didn't contribute anything really to my own support or tuition. There were very small scholarship awards to be earned at Purdue; if you made a high point grade average you got — I don't know — $25 refunded on your tuition or something.

**Sopka:**

You remained as an electrical engineering major throughout your undergraduate years?

**Purcell:**

Yes.

**Sopka:**

But you began to be aware of physics.

**Purcell:**

Yes. It was an interesting time at Purdue and in the history of Purdue science and Purdue physics especially, because in the years before 1929 when I came, physics at Purdue as physics was essentially negligible. The person in charge was a professor named Ferry, whose sole distinction consisted in having written a small book on gyroscopes. He was a lecturer of incomparable dullness. Lark-Horovitz came I think in 1929. I wasn't aware of his presence until the next year…

**Sopka:**

I wonder who was responsible for his coming. Did you ever learn the circumstances?

**Purcell:**

I don't know the circumstances. He was from Vienna, but a year or so before he'd been an NRC[1] fellow; he had some kind of fellowship I think at Chicago. There is some history of him and of Purdue that I hope someone … many, many people are competent to record that, and it certainly ought to be somewhere in the Institute files because his coming

to Purdue was really quite important for American physics in many ways. It was he who subsequently over the years brought many important and productive European physicists to this country; they came to Purdue, passed through. And he began teaching; he began having graduate students and teaching really modern physics as of 1930, in his classes.

**Sopka:**

That was an exciting time.

**Purcell:**

Yes, I only began to learn about it as I went on in my third and fourth year at Purdue. In fact, my first course in physics at Purdue was extremely dull. It was given by Ferry and I can remember almost nothing about it. I did have a good section instructor — I suppose he was an assistant professor; he had just gotten his Ph.D. and I found him interesting because he clearly knew physics at a level that I hadn't even come in contact with.

**Sopka:**

Do you recall what text you used as a freshman at Purdue in physics?

**Purcell:**

Oh, I think we used Ferry. I believe Ferry had a text. A dull book, but with an enormous number of routine problems.

**Sopka:**

Were there even then before Lark-Horovitz's arrival a sufficient variety of physics courses to allow a major in physics at that time in Purdue?

**Purcell:**

No, you couldn't major in physics I believe. You see, Purdue had electrical, civil, mechanical and chemical engineering. It had something called the School of Science, and you could graduate, having majored in science. I was never quite clear on the requirements of that major, but it was definitely below the others in its intellectual demands. In fact, it was a sort of refuge for those who couldn't make it in engineering. I think there you took a number of

different courses and probably were prepared to be a science teacher in the secondary schools or something like that.

**Sopka:**

But then Lark-Horovitz must have been given a fairly free hand to develop…

**Purcell:**

He had graduate students. He already had graduate students. That was the amazing thing. I don't know just how that sprang into being because the notion of a graduate student under Ferry was really… But there were a few other people who… Well, to come back to physics and my contacts. I was very happy with electrical engineering. I had a lot of fun and really found I could do it well and I really enjoyed the electrical engineering courses. But I guess it was when I was a junior I found in the catalog a course listed under the physics department called something like 'independent laboratory work.' I went around to see about it. No one had ever signed up for that course before, but they let me sign up for it, and my immediate supervisor was a professor named Walerstein to whom I owe a tremendous amount. He

was a real physicist. I think the first job he gave me was up in the attic of the physics building where there was a Rowland grating and a mount that had not been used for a long, long time. I was supposed to set it up and get it going and adjust it and everything all on my own and examine some spectra. Then he had some other things for me to do up in the attic, including making an electrometer to measure the halflife of something, and that was just absolutely… I was really hooked at that point.

**Sopka:**

About how old a man was Walerstein at this point? Was he a young instructor?

**Purcell:**

He was youngish and he stayed at Purdue for decades thereafter. He must have retired by now. But I should think he was in his thirties at that time, and there were others there. Well, that was I guess my junior year. And then my senior year I wanted to keep on there, and I was allowed to work with H.J. Yearian who was not a professor; he was still technically a graduate student. He was finishing up his thesis. He was doing electron diffraction. He was

already a tremendously experienced physicist. Why he was so long getting his Ph.D. was to be explained not at all by any lack of ability but by the way Lark-Horovitz was running things at that time. Yearian had built a 20-kilovolt electron diffraction camera, a Debye-Scherrer transmission camera. And he was going on to build another one and he let me work with the first model, and we began doing electron diffraction on various things — beryllium oxide among other things. And this introduced me to the whole world of the basement of the physics building where research in electron diffraction and x-ray diffraction was going on. These reflected Lark-Horovitz's own interests, and they were, of course, very much in the forefront of laboratory research at the time. People were also living down there in the cellar, sleeping on cots in the research rooms, because it was the Depression and some of the graduate students had nowhere else to live. I'd come in in the morning and find them shaving. Yearian was very nice to me, tolerated my mistakes and taught me a great deal. That was such an exciting time. I'd never really developed photographic emulsion before. Cameras had never been my hobby. I'd never done anything like that. And the

first photographic emulsion I ever developed, when I turned on the light in the dark room, I had Debye-Scherrer rings on it from electron diffraction — and that was only five years after electron diffraction had been discovered. So it really was right in the forefront. And as just an undergraduate, to be able to do that at that time was fantastic.

**Sopka:**

Between satisfying your requirements as an electrical engineering major and your taking physics because of your interest, did you have any time to take courses while you were in college that were what we call general education courses?

**Purcell:**

Oh, yes. You know, I find people are often surprised to learn that at Purdue at that time you could take a lot of other courses. In fact, my most serious and demanding outside activity was connected with the English department. I had several courses in English; I had a course in European history, a course in economics. I had just in terms of the course subject, not perhaps in the quality of instruction, I had as

much in the way of distribution courses as many graduates of Harvard College.

**Sopka:**

Did you have language training available?

**Purcell:**

I had two years of German at Purdue. I was very much involved with the literary magazine. In fact, I was editor of that for two years and that took a lot of time. In fact, aside from the people in physics, of whom Walerstein and Yearian were the ones I mentioned, the teacher who was the most influential and the most admired by me was in the English department, a professor named Paul Fatout. I was concerned with writing and I liked to try to write, and these people were interested in good writing. That took a lot of time.

**Sopka:**

Did you form any close friendships with any of your fellow students that would be the counterpart of your friendship with Dunlap McNair in your high school years?

**Purcell:**

Yes, I had many friends among my electrical engineering colleagues. All of us who enjoyed engineering enjoyed problems and things and arguments and jokes and the whole life of being an engineering student, and there were many like that. I might mention one in particular since he's not unconnected with the American Institute of Physics. Another person in my class of electrical engineering was J.G. Adashko. He was really a very close friend at that time. I haven't seen him for many years, but he has been the main translator of the Russian physics articles for the American Institute program for translations. He lives in New York. He was an electrical engineer in my class.

**Sopka:**

Was this very much of a masculine society in the engineering division at Purdue in those days?

**Purcell:**

Well, there were almost no girls among the engineers or perhaps one or two. I believe there was one girl in civil. And Purdue had coeds most of

whom were in home economics and made up not more than 15% I should imagine of the student body, so there was an enormous … It was almost like the old-time Harvard-Radcliffe ratio. I lived in a fraternity at Purdue the last two years. I lived in one of the regular fraternities. In retrospect I don't think it was a good idea. I think fraternities are and were terrible and should have been abolished. But at Purdue then there weren't so many dormitories. I would have been actually happier if there had been the kind of dormitory life there was many years later. I read now that fraternities are coming back at some of these places and I think that's deplorable. But that didn't affect my intellectual life and my growth in engineering and science. It was kind of separate from that. Lark-Horovitz ran the physics department on the European style: a pyramid with the professor at the top and everybody down below taking orders and doing what the professor thought ought to be done. This made working for him rather difficult. I was insulated by one layer from that because it was people like Yearian, for whom I was working, who had to deal with the Lark. In fact, it was he who really helped me go on as a physicist by helping me get a scholarship that I had for a year

after Purdue. So there's no question that I owe Lark-Horovitz a great deal. He was a remarkable man. You see, he came from a liberal Central European intellectual …

**Sopka:**

He was Jewish, too, wasn't he?

**Purcell:**

Yes.

**Sopka:**

There were probably not very many of them in the academic community at Purdue.

**Purcell:**

No, and the whole Middle West at that time was as full of anti-Semitism as any place you could find. Not only that, but he was politically liberal. And yet he managed to hold his own in that setting.

**Sopka:**

Was he married? Did he have a family to fit into the community as well as himself?

**Purcell:**

Yes. In fact, the hyphenated name — Lark-Horovitz, Lark was the name of his wife. That is, when they got married, they hyphenated their name. His name was Horovitz before he was married. And Betty Lark — she was a remarkable person herself. They loved music and arts and they were sort of a little island of Middle European culture there in central Indiana.

**Sopka:**

About how old were they when they came? Had he recently finished his own doctoral work in Vienna?

**Purcell:**

He'd been a post-doc at Chicago, as I recall.

**Sopka:**

Was he under 30, would you guess?

**Purcell:**

Well, no, he must have been at least 30. Of course, everyone seemed old to me then. Thirty now seems ridiculously young.

**Sopka:**

In order to have stepped in and become the top of the pyramid, he must have had some seniority.

**Purcell:**

He must have been in his thirties already. I have a marvelous picture of him complete with spats taken at that time. Imagine wearing spats at Purdue. His own scientific interests more and more led him in the direction of what we now call solid state physics. He was interested generally in crystals, the physics of crystals, and that sort of thing; and of course later on he developed the really very well known and very important solid state research laboratory that did work on semi-conductors during World War II. The trouble with the Lark was he was very slow to write things up and publish them. I think perhaps he didn't get as much credit as he might have for his work on germanium in the middle '40s prior to the transistor.

**Sopka:**

You mentioned then that he was instrumental in helping you to get the opportunity for the year you spent at the Technische Hochschule at Karlsruhe.

**Purcell:**

Yes, I'm sure that his recommendation was what made that go through. You see, actually my first published paper was on work I did at Purdue as a senior. If you were doing electron diffraction, part of the game was to get a film of the material thin enough to be clearly transparent to electrons; and nowadays it's not much of a problem. But Lark-Horovitz had an idea about how to do that, which he put me to work to try to carry out, which was to evaporate the material that you wanted to study onto a substrate which was of some material which would sublime, evaporating without turning into a liquid in the vacuum, because ordinarily if you evaporated onto anything, the surface tension when it melted or anything would tear a film. So I spent a long time figuring a way to evaporate copper onto napthalene or mothballs. What we did was to make a slit, fill it with napthalene, and polish the base of it, evaporate copper, and then put it into a vacuum, keeping it very cold all that time. And then after you got it into the electron diffraction camera, you let the stuff evaporate into the vacuum and the mothballs just sublimed, leaving the film in pretty good shape. So there was a little paper on that in RSI,[2] of which I

am one of the co-authors. I think that it turned out, in the end, that what we had was copper oxide rather than copper. Somewhere along in this process it had, gotten oxidized.

**Sopka:**

So by the time you had graduated was your leaning definitely toward a physics career?

**Purcell:**

Yes, I decided to go into physics. It was, of course, the Depression; and whether I could have gotten a job as an electrical engineer anyway is doubtful. I don't remember that being the overriding reason somehow. I can't remember worrying about it. By then I really wanted to go on in physics, and I'd been really fired up by those experiences.

**Sopka:**

Under what program was the fellowship?

**Purcell:**

The Institute of International Education ran the exchange fellowships then and does again now. They ran them in different countries, but the German

program was a particularly active one., German students came to this country for a year, and American students were sent to Germany for a year.

**Sopka:**

Was that inaugurated not only for the academic exchange but also for the cultural exchange in terms of say binding up the wounds after World War I kind of a program?

**Purcell:**

I honestly don't know. I've seen something about the history of it lately. I think the thing got started about 1925 or '27 maybe — I'm not sure — but it hadn't been going many years.

**Sopka:**

I know that when Arnold Sommerfeld came to this country he said that he was coming more to try and be an ambassador of good will as much as to share the physics… He felt very strongly about the anti-German feelings that had come into being.

**Purcell:**

Yes, but that was a little earlier, wasn't it?

**Sopka:**

Yes, that was 1923 or '24 he was here.

**Purcell:**

That's right. I don't know where the money came from for the Institute. Not only Germany was in this program — I think France was, too, and others. But we had had just in my last year at Purdue a German student in engineering, and I had gotten acquainted with him somewhat — as it turned out, a man with a very remarkable future. His name was Fritz Leonhardt. Well, Fritz Leonhardt was an exchange student. He was a young German civil engineer, structural engineer, who then went back to Germany; and then when I was in Germany as an exchange student the following year I visited him. He later became the leading proponent and innovator in Germany of prestressed concrete construction and built some spectacular things. Then much later in life he became I think Rector of the Technische Hochschule at Stuttgart, among other things. But his home was in Stuttgart. He had been at Purdue as an exchange student on this international exchange the year before, so I knew him, met him at Purdue, and

then I looked him up when I was in Germany after he'd gone back and was in Stuttgart again in '33.

**Sopka:**

About what kind of a budget were you allowed for that year in Europe?

**Purcell:**

Oh, very slight. I had free tuition and I had free board and room at the Studentenhaus. And that was it more or less. I may have had some small stipend in addition but very, very small. Things were very cheap at that time.

**Sopka:**

And presumably your travel costs …

**Purcell:**

It cost me, or cost my family, a total of $700 for the whole year, including travel. No, it was rather an austere life. I didn't have enough money to eat meals except the free meals at the student mensa.

**Sopka:**

Was this a hardship? Was the change of diet appreciable?

**Purcell:**

Well, no. We survived perfectly well. It was just not luxurious at all. It was of course a very bad time to go to Germany, the beginning of very bad times. Hitler had already come into power a few months before. So through the year I had very few friends among the German students, who were already most of them caught up in the movement. My friends there were from other European countries who had come to study engineering.

**Sopka:**

So were you still studying engineering rather than physics?

**Purcell:**

No, I was studying physics. You could study physics there. And there was one real professor, Walter Witzel, a young vigorous, excellent teacher. His own interest was in band spectra. In fact, I think he had

just written the volume on band spectrum in the HANDBUCH DER EXPERIMENTALPHYSIK. And he, however, had been kicked out by the Nazis just before I got there because he'd been a socialist or communist as a student once or something, and for the first semester he wasn't allowed to teach. But he was allowed to come back in the second semester, and I had then some lecture courses from him, graduate courses. He was a wonderful teacher at that level. He went to Bonn a few years later where he served out his entire academic career. In fact, I've seen him and visited him in Bonn after the war. But it was not a very good place to go. I had no choice. I would have much preferred to go to Munich where Sommerfeld was teaching and where many other professors were.

**Sopka:**

Were you assigned?

**Purcell:**

I was assigned, yes. I had no choice.

**Sopka:**

Were your two years of German at Purdue adequate for you to get along both socially and academically in classes or was it a struggle?

**Purcell:**

Well, it was a struggle. That was part of the fun really. Some of the exchange students spent a month in Hamburg early in the fall just studying language, which helped in many ways, and I enjoy foreign languages, and in fact during the semester that Witzel was not teaching we used to go around and see him at his apartment. But I took all kinds of courses then in other subjects; because I had free tuition, I could take anything I wanted. So I took several courses for the purpose of hearing that professor speak German if he was a particularly elegant lecturer. There was an historian at Karlsruhe named Schnabel, a marvelous scholar, whose lectures were such beautiful lectures. I took his lecture course and I took a seminar with him. I went to a course in the history of art, standard type lantern slide history of art course; and I took a course in physical chemistry.

**Sopka:**

In general did you find the European academic atmosphere more sophisticated than what you had been exposed to at Purdue?

**Purcell:**

Yes, they were more sophisticated in many ways. The student life, of course, was quite different. As is well known, they didn't have exams as we did — regular, frequent exams. And I also found that my colleagues in the graduate course — you might call it sort of a first-year graduate level course in physics — these people hadn't had much practice working problems, whereas of course I'd been doing nothing but working problems for four years. So although my mathematical preparation was not as good as theirs and my physics in some ways not as good, I was better at working problems. My mathematical preparation I might say was still rather weak and was to remain so throughout the rest of my life. That was one respect in which I came out of Purdue not too well prepared. Well, I had calculus and differential equations in the engineering style, but I

didn't really find out what mathematics really is until I came to Harvard.

**Sopka:**

I guess if you were on such a limited budget, you didn't have much chance to travel in Europe and with the political atmosphere in Germany…

**Purcell:**

Well, I traveled in Germany because it was very cheap. You see, if you were a registered student in a university in Germany, you could travel, get all kinds of reductions in your travel costs. And we, of course, exploited that very much. No, I traveled a fair amount and did some hiking and things of that sort. I went into Austria all alone to ski at the time Germans were forbidden to go there. But it was a grim time for Europe because the grisly Nazi era was right ahead and beginning to be obvious.

**Sopka:**

I believe you told me one time that you met your future wife during this year.

**Purcell:**

Yes, yes, that's right. She was also on an exchange scholarship. She was sent to Munich. Her field was German literature.

**Sopka:**

Where had she gone to college?

**Purcell:**

Bryn Mawr. We met going over on the boat and then at Hamburg that time, and I visited her in Munich. She went around to hear Sommerfeld lecture in Munich. I said she should go hear him and at least listen to Sommerfeld. I remember going to a German physical society meeting. That just came back to me. I hadn't thought of it for a long time. It was in what must have been the early spring of '34. It seems to me this meeting was in Freiburg. And everybody was sitting around and laughing about the neutrino idea. I remember sitting around a table drinking beer and the senior physicists joking about the neutrino.

**Sopka:**

When your year in Europe came to an end, did you go back home or did you come directly to Harvard?

**Purcell:**

Well, I had to get a scholarship of course before I left Germany. So I wrote letters and again I think Lark-Horovitz helped me there and wrote to two or three different places for a graduate scholarship and got a reply from Harvard that said (I forget what they called it) … but it was just free tuition. I think it was $400.

**Sopka:**

That's what it was in those days.

**Purcell:**

That's what it was, and you never saw the $400. It was just canceled out against your tuition. That wasn't a terribly munificent scholarship, but that was the best one and I decided to take that. I had to consult my parents and see if they thought they could swing the rest of it. So before I left Europe I

knew where I was going to be the next year. I was coming to Harvard.

**Sopka:**

When you actually arrived in Cambridge did you go into one of the graduate houses here on Oxford Street?

**Purcell:**

I lived in Perkins the first year up on Oxford Street- and Conant the next two years.

**Sopka:**

Presumably you had to pay for room and board there from your own pocket.

**Purcell:**

Yes, that's right. I had to pay the Perkins room out of my own pocket.

**Sopka:**

There wasn't any proctorship or something like that that you could earn your way?

**Purcell:**

No, I guess there were such things, but I didn't have one.

**Sopka:**

Probably not for a first-year graduate student anyway. It might have come later.

**Purcell:**

No. So I was a full-time student, didn't do any teaching until another couple of years… Well, the Harvard graduate student in those days up in this part of the University really was quite disconnected from the life of the college and everything that went on in the houses and the yard and everything. I mean that almost was nonexistent as far as we were concerned. We were working very hard and working up in this vicinity, eating in restaurants up and down Mass. Avenue.

**Sopka:**

You mentioned before that when you came to Harvard you learned mathematics that you hadn't been exposed to before. Would you want to

comment on what courses and faculty members stand out in your memory?

**Purcell:**

Well, the great thing that stands out in my memory was Math 13, as it was called then: complex variable theory.

**Sopka:**

Who taught it that year?

**Purcell:**

David Widder. I took it my first year here as a graduate student, and it was a tremendous experience. It was really one of the intellectual experiences of my life. It was also very nearly my Waterloo, because I could easily have flunked out. But I had never seen mathematics, a course from a mathematician's point of view. The whole idea of what a proof really is was totally new to me. All this business with epsilon and delta, you know, and those arguments: an absolutely new idea. And I worked very hard. It also showed me almost immediately that I was certainly not cut out to be a mathematician because in that class there were graduate students in

mathematics who were clearly operating on a very different plane from mine. I went to the exam the first semester, the final exam, in January, and I had worked tremendously hard. The night before, 1 had proved every theorem in the course; and I went to the exam and simply froze. I couldn't do anything. I couldn't solve a quadratic equation.

**Sopka:**

I know exactly how you felt. That's happened to me, too.

**Purcell:**

I remember exactly where I was sitting over in the geology lecture room. I can almost pick out the seat over there but they've probably got new seats in the room by now. And I sat there for three hours and I couldn't do anything, just miserable little scribbles. It's never happened to me since, but it's always made me sympathetic to students who come around and say, "Gosh, I couldn't do anything on that exam."

**Sopka:**

How did you manage to salvage that experience?

**Purcell:**

Well, in those days it was a full course, you see, and your grade for the first semester went in in pencil, and then your final grade. So my first semester grade was I think C-, which was as low a flunking as you could get for a graduate course in those days. But I had done all the problems and everything. I had a good record otherwise, and in the second semester I just went at it again and pulled it out. I don't think I got an "A" but I at least... Stayed respectable. But quite apart from that, just the whole depth and vision of that mathematics as seen from classical analysis, which is of course a beautiful subject in its self-consistency and completeness. On the other hand, there are still many parts of modern mathematics that I didn't get all along the way. Modern algebra: I never really had anything in that, which of course handicaps one in a way one is particularly conscious of in recent years.

**Sopka:**

Math 13 then was the only course you took outside of the physics department?

**Purcell:**

Right, yes. Well, the only mathematics course, yes. I audited a course in cosmology by A.N. Whitehead, who was still here then.

**Sopka:**

Oh, he was around at that time?

**Purcell:**

Yes, a beautiful gentlemen. I didn't get very much out of it. I did some of the reading.

**Sopka:**

Under which department was he?

**Purcell:**

Philosophy.

**Sopka:**

But then within the physics department itself you must have taken some courses.

**Purcell:**

Oh, yes, I took lots of courses … Well, I took courses with Kemble. That first year I guess I had electricity and magnetism, classical E & M, with Kemble; 32 I believe was its number.

**Sopka:**

Yes, there was a whole sequence of courses in the '30s — in the 1930s and the sequence was labeled the 30s.

**Purcell:**

And then Kemble's quantum mechanics. And I had a course with Furry — I don't know what, I can't remember exactly what that was called.

**Sopka:**

Introduction to Mathematical Physics or something like that?

**Purcell:**

Well, something like that was given, but I think I did not take that. I'm not sure anymore. I've forgotten. Oldenberg's lab course I remember.

**Sopka:**

Did you take that?

**Purcell:**

I took that.

**Sopka:**

That was a nice one.

**Purcell:**

That was a nice course. In fact, for me again that course was something so new and lovely in that we didn't have to write lab reports, and here at Purdue, God, as an engineering student, I had to write lab reports all the time. And I don’t like lab reports, and here was a course … I remember Oldenberg's practice was: you just put the result down on a little card, a 3" by 5” filing card, and you wrote down the result of your experiment on it. As a physicist, you see, I've never kept a laboratory notebook. I don't think I've ever had one.

**Sopka:**

So you won't have to worry about which archives will get your … laboratory notebooks.

**Purcell:**

No, I have nothing for the archives.

**Sopka:**

When did you begin to do individual research that would lead to a doctoral thesis?

**Purcell:**

Well, I can't remember how soon it began chronologically, but I remember the subject involved. Professor Chaffee gave me a thesis problem, and I began with the idea of doing a thesis under him. And not only did he give me a problem, but I had a room, a room actually right below this, to start work in; and I started building apparatus. I had the whole room to myself. Down there now there are six fellows in that room. And then the problem he gave me was an excellent problem and would have been a tremendously fruitful thing, but I talked myself out of it and talked him out of it. Chaffee had

the idea that it would be interesting to look at a gas discharge in a magnetic field and then put in a couple of condenser plates in the gas discharge and measure the AC impedence of those plates as a function of frequency. And if you did that, wouldn't you see some kind of an anomaly at the cyclotron frequency — what we would now call the cyclotron frequency of the ions? And if you did, couldn't you use that as a mass spectrograph? See, by going through the cyclotron resonance, we would now say in modern language, "We're looking for the cyclotron resonance in a plasma, or at least a partially ionized medium, not a real complete plasma." So I started out to do this experiment. I had the coil to make the field and built the thing. And then I began doing some calculations which were very discouraging, and I went to Chaffee with them and convinced myself and convinced him that the thing wasn't going to work. Well, it just shows you. Actually, the calculations were correct, but not completely relevant, and I fell quite sure that if I or anyone else had gone on energetically with that idea experimentally at that time, he would have discovered the trick he needed to do to make it work. In which case he would have had what was called

the Omegatron ten years later. He would have invented that well ahead of its time. And I simply calculated myself short-sightedly out of that. Well, that evaporated then, and in casting about for something else I went to Bainbridge, and Bainbridge suggested that I might try something with tracks in emulsions, which was just developing at that time: nuclear tracks in emulsions. So I spent some time trying to see if I could see beta ray tracks in photographic emulsions.

**Sopka:**

What kind of a source would you have been using?

**Purcell:**

I used old radon tubes. We had to get the plates. They were Ilford plates from England, and they were plates that you could see alpha tracks in easily; and the game was to expose these to beta rays going in more or less tangentially into the emulsion so there would be a long track and then look for electron tracks. Well, it was really hopeless, as I really should have been able to calculate. But this time instead of calculating myself out of it, I spent quite a little time actually looking for the beta ray tracks and didn't see

any. And in retrospect it's clear that I should not have seen any; that the emulsions at the state of perfection they had reached then, you could not see a minimum ionizing track. The background was much too high. Then Bainbridge suggested what became my thesis topic of looking at the focusing of charged particles in a spherical condenser. At that time of course everybody was all full of the lore about the focusing in magnetic fields, including the sector focusing, you know, where you have a sector of a magnetic field, which in fact was one component of Bainbridge's mass spectrograph and the mass spectrograph of Aston and others. So I did a little calculating on that — it looked very interesting. I set out to build an electron model, so to speak, to show that electrons would be focused by such a device, and to make it actually run. Again, it isn't clear why building it was a useful exercise except to get a Ph.D. thesis; there was nothing dubious about the equation of motion for the electron in the electrostatic field. Nevertheless it was a great experience to try and make this thing, which I finally succeeded in doing. It wasn't good for anything. That is, I was just focusing ordinary thermionic electrons to show that it would work. I wasn't

measuring anything that needed to be measured. On the other hand, in working out the theory I did discover an interesting general property of the spherical electrostatic analyzer — the equivalent of what was known as Barber's rule for magnetic sector focusing. The spherical electrostatic analyzer is used now, oddly enough. It's been revived after lying dormant for 30 years and is now used in slow electron surface work. Its advantage is its big solid angle. We weren't so rushed in those days and spending that much time fooling around trying to find a thesis was not so unusual. And there was more space to work in.

**Sopka:**

You got your degree in '38?

**Purcell:**

Thirty-eight, yes.

**Sopka:**

Had you spent about two years actually on this thesis topic?

**Purcell:**

Well, no, it was less than that.

**Sopka:**

Because you were four years as a graduate student, and the first one you were presumably taking courses full time…

**Purcell:**

But on the actual spherical condenser topic, I think I spent not more than a year and a half.

**Sopka:**

Was it during this period that you did get married?

**Purcell:**

Yes. I was married in '37.

**Sopka:**

I see, which meant that you moved out of Conant or Perkins, whichever hall.

**Purcell:**

Conant, yes. And my wife got a job teaching out at Milton Academy, so we lived one year out in Mattapan and then two years up here in Cambridge near where we live now.

**Sopka:**

What had your wife done between the time when she was in Germany and …

**Purcell:**

She went back to Bryn Mawr and did more graduate work.

**Sopka:**

I see. Did she ever complete her own doctorate?

**Purcell:**

No. She took graduate work in philosophy. She wanted to take more philosophy, and she did not complete a Ph.D. and she was teaching at Bryn Mawr also.

**Sopka:**

It's now the afternoon. We've taken a luncheon break and we'll pick up again with our discussion of Professor Purcell's years as a graduate student.

**Purcell:**

One of the very important chapters in my education as a graduate student in physics is associated with Professor Van Vleck. This had nothing to do with my thesis work, but beginning in perhaps my third year in graduate school in I suppose it must have been '36, I took Professor Van Vleck's course in electric and magnetic susceptibilities, which is the subject, of course, of his famous book. There were only two people who signed up for the course — Malcolm Hebb was the other one. Malcolm had been a graduate student of Professor Van Vleck's at Wisconsin — I guess I mean Wisconsin, don't I? —

**Sopka:**

Yes, he came here from Wisconsin.

**Purcell:**

— and had come with Van from Wisconsin a year or two earlier. Malcolm was a theoretical student who knew already a good deal of theory, which I did not, but during this one-semester course where I learned a great deal of quantum mechanics, among other things (many people have learned a lot of quantum mechanics from Van's book), Van assigned as a term problem a consideration of the theoretical aspects of cooling by adiabatic demagnetization, which was then a rather new development in low-temperature physics actively pursued at Leiden and at Oxford. We started working on this as a term problem. In fact, we worked on it for the following year, and our results were eventually published in a joint paper by Malcolm and me and a companion paper by Van Vleck himself. This was a real turning point in my development as a physicist. I didn't contribute too much to the work. I really didn't understand extremely well what was going on in the calculations I was doing. But Malcolm did and of course Van understood it with crystal clarity, and our paper was the first substantial theoretical paper on magnetic cooling and was widely referred to later by people working in that field. In the course of it, through

Van's generosity and help, I remember becoming acquainted with R.H. Fowler who had come over on a trip that year and was interested in things like this. And of course, as always, Van treated you as if you understood what was going on as well as he did, and he continues to speak to me about that paper as if I understood what I had been doing.

**Sopka:**

Was it an entirely theoretical undertaking? Or was there any attempt to follow up with …?

**Purcell:**

No, it was entirely theoretical. You see, at that time there were only two places in the world where any experiments could be done — Leiden and Oxford. They were the two low-temperature laboratories. Well, there was some work at Toronto. And Giauque at Berkeley was doing some experiments and should be included, too. The data that we were trying to fit finally with our calculations I think mainly came from Oxford, if I recall properly, from Simon's laboratory. That may not be correct. But at any rate, this was an introduction to some physics that I was

going to have to come back to again and again in my later work in nuclear magnetic resonance.

**Sopka:**

That was all part of the course that you were taking? Or was that an extension beyond the course?

**Purcell:**

Beyond the course because the course ended … It was a one-semester course and it had a final exam…

**Sopka:**

For two students?

**Purcell:**

For two students. In those days, of course, Harvard, as you remember, Harvard exams were printed, set up in type and printed and there was a serial number up in the corner, and I guess Malcolm had # 01, and I had # 02. I don't know how they stopped the presses quickly enough. No, we went on working then for roughly a year after that on our term paper, writing it up and working out the details of the calculations. My half of it was concerned with iron-ammonium alum and Malcolm's was cesium-

titanium alum, the two substances that the experimenters had used in their adiabatic demagnetization cooling. Well, after I got my Ph.D. in '38, the Harvard cyclotron was being put together and my research time was really turned in that direction, and with a number of other people, under the leadership of Professor Bainbridge, the pre-war Harvard cyclotron was built.

**Sopka:**

Was there any question about your staying on immediately? Did you consider going elsewhere or was the opportunity to stay on at Harvard so attractive …?

**Purcell:**

I stayed on just as an instructor, I guess, whatever the lowest rank was, I believe… Well, the first year or two must have been just as an instructor. But then the faculty acquired a rank called Faculty Instructor.

**Sopka:**

Instructor of Physics is the first rank. And then comes Faculty Instructor.

**Purcell:**

Faculty Instructor — that's right.

**Sopka:**

Which apparently was almost equivalent to assistant professor, because there wasn't assistant professor.

**Purcell:**

For some reason they temporarily abandoned the title of assistant professor and introduced this faculty instructor, which, as I recall, was a five-year appointment. And so the war years I spent as faculty instructor on leave. But faculty instructor meant that people in that category could vote in the faculty meeting I believe. Well, so we worked on the cyclotron until it got running in '39. My contribution to that was work on the magnet power supply and controls mainly. People I worked closely with were Jack Livingood and Rubby Sher, now at Princeton; Binx Curtis. Then in the fall at the end of 1940 the Radiation Lab at MIT started up and before the end of that year I was committed to go down there.

**Sopka:**

While you were here as the lower level instructor, in addition to your cyclotron building activities, did you have teaching duties?

**Purcell:**

Oh, yes.

**Sopka:**

How heavy were they?

**Purcell:**

Well, I don't remember … I don't think they were terribly heavy. You see, I had begun as a teaching fellow in 1937 before I got my Ph.D., and I'd worked in old Physics B under Professor Black. It seems to me that in the late ‘30s I remember working with Professor Saunders in what we called then Physics C. I don't remember too well what courses I was doing in that period of ‘38.

**Sopka:**

You were in assistant capacity rather than having the responsibility for working up your own lecture material.

**Purcell:**

Yes. I think the first time I gave a lecture course on my own responsibility was … possibly in '39, but I'm not sure of that. It's not very important in any case.

**Sopka:**

I was wondering how your time tended to be divided in those post-doctoral years in terms of your cyclotron commitments and your teaching commitments.

**Purcell:**

Oh, it was roughly 50-50, and we had tutorial then, so that I had a number of tutorial students, undergraduates. I shared an office with Jack Livingood. The interesting physics course that was created in those years was Physics F and G, which was a sort of really advanced two-year physics

course, which, as it turned out, has as undergraduates many people who later became well-known physicists, including some in our own department. But I had nothing to do with that, although in sharing an office with Jack, who did, I kept following it. I think I was engaged perhaps with Physics B at that time. In fact, I may have been responsible for Physics B in '39, because Black was ready to retire. Now, so after the cyclotron got running and then came Radiation Lab at MIT and the threatening war clouds ahead, but I went to work full time at Radiation Lab I guess in January, at the end of the first semester of ‘40-41, having gone a little part-time before that.

**Sopka:**

Who recruited you to the Radiation Lab, do you recall?

**Purcell:**

I would say it was Bainbridge, who was one of the original group. Bainbridge, Rabi and others were in on it. When I went to Radiation Lab, I went into Rabi's division. We were just really in the process of forming the divisions then. But Street and

Bainbridge had already gone down. Ramsey wasn't at Harvard yet. He was there from Illinois. And Zacharias was there. Lee Du Bridge was, of course, the director. And physicists were gathering from all over the country many nuclear physicists: McMillan, Alvarez from Berkeley, Ray Herb from Wisconsin, Pollard from Yale, Milton White from Princeton …

**Sopka:**

Was it immediately clear what your mission was at the Radiation Laboratory?

**Purcell:**

Yes. Well, you mean what the laboratory's mission was?

**Sopka:**

Yes.

**Purcell:**

Yes, in fact it was extremely definite. Our mission was to make a radar for a British night fighter using 10-centimeter magnetron that had been discovered at Birmingham. And from that narrow assignment grew an enormous breadth… But our first

assignment was very definite, which was probably a good thing. We didn't know how to do it, mind you. I mean, there were many components of the radar that had not even been invented yet, but their necessity was already recognized.

**Sopka:**

Did you then spend the entire war year period in Cambridge?

**Purcell:**

Yes. I was in Cambridge the entire war years, including clear up into '46 because I stayed to help write some of the books, the Radiation Lab books. But I was there all that time — five years, more or less. I spent one week at Los Alamos shortly before the Trinity Test at Alamagordo, helping to design some of the equipment for that test. I was imported to Los Alamos as a transmission line expert, so to speak, by Bruno Rossi, and in that way learned what was going on at Los Alamos. By then many people from Radiation Lab had gone out there, including Bainbridge, Ramsey and Rabi. Well, Rabi kept a foot in both places throughout the war. He went back

and forth. But others — Alvarez, McMillan, Ramsey — had gone out full time.

**Sopka:**

Do you remember what the response of the people here at the Radiation Lab was to the word of what really was going on, the atomic bomb, when the information was released? Was this a total surprise? Did people react …?

**Purcell:**

Well, how should I say it? For one thing, as I recall it, we were preparing a very elaborate news release on radar, which had been kept very secret, and it was decided that there would be a general news release on the nature of radar telling everything. People had been working for weeks getting all this ready, and the release date for this was just the day that the atomic bomb news came out, so…

**Sopka:**

You were snowed?

**Purcell:**

We were somewhat snowed, yes. I don't know. I myself, just as one example — we all knew they were working … You know, there was a big effort going on out there that had to do in some way with uranium fission. When I went out there whenever it was in February of '45 or something like that, I was surprised to find it was a bomb. I thought it was probably going to be power, you know, an energy source. So I didn't guess correctly, but maybe others were better guessers than I was. And we knew, of course, that there was an enormous, big thing going on. There were many people, of course, like Rabi who knew the whole story.

**Sopka:**

In recent years there has been so much discussion and retrospective criticism of the scientific community for the development of the atomic bomb in the '40s. We're interested in knowing if people can recall how they and their groups felt about war work in general and about the atomic bomb in particular.

**Purcell:**

Yes. Well, as far as war work in general is concerned — let's say at the Radiation Laboratory where we worked on radar during World War II — there was really not the slightest discernible feeling that we should not be doing this. Everybody, so far as I know, completely accepted the proposition that it was a matter of desperate urgency to beat Hitler. I suppose in particular types of weapons a question might have arisen: even to beat Hitler is it right to do so-and-so? But certainly in the use of radar that question never arose. Well, you might say it could have, because one of the uses of radar in the latter part of the war both by ourselves and the British, of course, was for blind bombing by the heavy bombers of Bomber Command and of the American Air Force. In retrospect one might say the firebombing of Dresden was as great a human catastrophe — indirectly assisted by radar — as you could imagine. But at the time no one would have really raised that. The situation was too desperate. Hitler was about to win. Also, in the beginning of the war, the place our radar really paid off first, I think, was in the anti-submarine campaign. Ten centimeter radar really was one of the very important weapons that finally

beat the German submarines. And the seriousness of that threat alone, those of us who knew what that was … In the summer of 1942 I accompanied Rabi to England to talk to various opposite members in the British radar development. It was at the headquarters of RAF Coastal Command that we saw for the first time (I saw — maybe Rabi had seen it before but I hadn't) the big map of the Atlantic with a pin stuck in for every sunk ship. Along the coast of the United States from Maine down to Florida … there was scarcely room for another pin. And Coastal Command at that point was really beginning to make some progress in killing submarines as they came across the Bay of Biscay on the way back to their home ports on the French coast. So, you know, there was no argument there about what you'd better be doing in fighting these bastards. There was great concern at the Radiation Laboratory after the war — and this is I think an interesting chapter on all that debate, you know, about setting up the Atomic Energy Agency. Should it be under civilian control or military control? And that whole argument about the McMahon Act and stuff. And the Radiation Lab people were by then — with the war pretty much over — much concerned about that. We had

meetings and discussions and committees and people signing petitions and so on and calling up Washington, very much in on the argument, which culminated in making the AEC a civilian agency.

**Sopka:**

I see. Were you personally involved in that, in those discussions or arguments?

**Purcell:**

No, I was not conspicuous in those arguments. I went to them and was involved in them, but there were a number of people who were the leaders in organizing those things. My own involvement at the end of the war in government matters grew in a different way. It came through — I guess through DuBridge. The Air Force was setting up a Science Advisory Board. Lee DuBridge was on it and took me along as a junior assistant. That was my initiation into the government advising activities that took a good deal of my time in later years. I was a member of the Air Force Science Advisory Board for quite a long time.

**Sopka:**

Did you find this kind of activity satisfying and did you feel that you were accomplishing something as a spokesman from the scientific community or was it difficult going that you felt that somebody had to do and …?

**Purcell:**

No, it really wasn't quite like that at the beginning. It was not so much that we were representing the scientific community at all, because we didn't identify then really any interests of the scientific community as such. It was that we had become rather knowledgeable, technical experts on this whole new military technology. The Radiation Lab people became accustomed to dealing with military plans and technology during the war and had many close associations with the problems. So these people needed our advice. For instance, one of the first tasks of the Advisory Board to the Air Force, which was run by the famous fluid dynamicist, Theodor von Karman, was to try to predict the nature of things to come technically and scientifically. I was involved in that. I'm not sure our

predictions were any good, but that's what we were set up to do. You see, the scientific people evolved during World War II entirely new relationships with military people, one of mutual confidence and understanding of the problems and working together, so that the Radiation Lab finally put itself in a very strong position with respect to scientific undertakings with the military for military problems. It wouldn't do anything just on order. It had to know the whole purpose and would attack the problem in the large but insisted on being in on that whole … The British had developed in the same way in complete contrast to what happened in Germany, where the scientists were never really trusted by the military or the government at all. They were used, some of them, and some of them like von Braun were used to do new things. But the relationship was totally different. Then, of course, one result of that on the other side was the creation of the Office of Naval Research after the war, which was the thing that kept research going in physics until the National Science Foundation finally could take over. But it was the Navy and particularly a few officers and a few civilians in the Navy who realized that the kind of research that we now needed to carry on in pure

science was going to be expensive and sophisticated and needed support. And the military at that point were the only ones that had the budget to support it. So the Office of Naval Research began right after World War II supporting pure physics research universities with no strings at all. And I think one can trace that to the working relationship that did develop between scientists and the Navy during the war, when we were right in there helping them solve their problems.

**Sopka:**

Apparently they did realize that they couldn't have gotten far without the scientific community's help.

**Purcell:**

Oh, yes. I mean the ship, except for its hull, was full of stuff that science had produced for them during the war. Well, let's see, I don't know whether we should say much more about Radiation Lab here. Many people will be talking about that in this series of interviews I'm sure. I might just remark that what I learned there helped my own career in physics. I really consider myself extremely lucky, and I mean lucky, to have been able to profit personally from

what I had to do there at the Radiation Lab. Not only did I learn a whole new armament, acquire a whole new kit of research tools — microwave technology, transmission lines, signal to noise theory, just a lot of different things, all of which were going to be useful one way or another — but perhaps the most important thing was being thrown together in a working relationship with a number of physicists from other places: in particular, physicists from Rabi's laboratory at Columbia. Here I'm thinking particularly of Ramsey and Zacharias and Henry Torrey; and then at Columbia still during the war, Kellogg and Kusch, and of course Rabi himself with whom I was very closely associated through all that time, Rabi being the head of the division to whom I reported.

**Sopka:**

Which division was that and what was your specific responsibility?

**Purcell:**

Well, the designation of the division was Division 4, Advanced Developments, I think. But it was an interesting collection of groups which operated a

little bit outside the pattern of some of the others and with a little more freedom to choose what they wanted to do.

**Sopka:**

Were you responsible more for developmental rather than perfecting techniques?

**Purcell:**

Very much so. In fact, we were responsible for exploratory things. The group I associated with longest was called the Fundamental Developments Group, and what we did, roughly speaking, was to make the first breadboard models of radar at a new wavelength, which meant that we had to develop a lot of the plumbing and the circuits and for the ever shorter wavelengths. The Lab started at 10 centimeters, and then our group was working on three centimeters, and then after that was well established and in production, we were working on one centimeter radar. We were also free to try and invent things and measure things that were relevant. There was no problem in keeping people at jobs that were in some way relevant. Another group in Rabi's division had to deal with microwave propagation,

which turned out to be a rather more complicated and subtle problem than had been anticipated. Another group under Jim Lawson was really responsible for establishing or perfecting and proving and teaching the theory of signal to noise as applied to detecting radar signals. And then there was the theoretical group, which was also under Rabi. Most of their theory was concerned with electromagnetic fields and signal to noise, things of that sort. George Uhlenbeck was in charge of it for quite a long time, and Bethe was in it for a while; Schwinger was in it; Frank Carlson; David Saxon, now president of the University of California; Goudsmit also. Came the end of the war and we were all thinking about what shall we do when we go back and start doing physics. In the course of knocking around with these people, I had learned enough about what they had done in molecular beams to begin thinking about what can we do in the way of resonance with what we've learned. And it was out of that kind of talk that I was struck with the idea for what turned into nuclear magnetic resonance. Another very important association there was with Bob Dicke, who was a member of my own group but who worked pretty independently. It was

during his work there that he invented the Dicke radiometer, which is the basic tool of radio astronomy, and applied it to measure molecular absorption. By the end of the Radiation Lab, in '46 even, Dicke had measured the absorption of water vapor in the atmosphere; working with Beringer he had measured the oxygen absorption at six millimeters; and he had observed a partial eclipse of the sun with a K-band radiometer. And, in fact, he had made a measurement of the sky temperature which set a non-trivial upper limit on the density of what we now know as the isotropic microwave background radiation, although I believe Dicke himself later forgot that he had done that.

**Sopka:**

This was done at the Radiation Lab?

**Purcell:**

At the Radiation Lab. That was measured from the roof of the Radiation Lab, yes. You see, toward the end of the war we had made a terrible mistake, and my group had contributed to it or I contributed to it certainly. Namely, we had picked the wavelengths to settle on for K-band almost on top of a water vapor

absorption line. This turned out to be fatal to the use of K-band for long-distance radar (for which it wasn't very good anyway). We were greatly chagrined as a matter of fact — it should have been possible to predict this by using some data that were already contained in a paper by Van Vleck. When we began to suspect that we were having trouble with water vapor, Dicke used his radiometer to measure the water vapor absorption line. This led, of course, directly into microwave spectroscopy, was indeed microwave spectroscopy. And the techniques developed during the war provided almost the basis for the explosive development of microwave spectroscopy after the war. There were other things that we thought might turn out to be interesting physics but didn't. One of the extremely important results was in semi-conductors, because the physics of semi-conductors had already been stimulated during the war through their use in the shape of crystals and crystal detectors. In fact, the Radiation Laboratory had been supporting Lark-Horovitz's program at Purdue on germanium as well as other more empirical, edisonian research on the properties of semi-conductors.

**Sopka:**

It would appear then that the existence of the Radiation Lab not only served the function of helping the war effort but that it brought together a group of scientists who in other circumstances would never have worked together, and that in this meeting of minds other things were begun that have later proved quite fruitful.

**Purcell:**

Yes. Oh, very much so. It worked in so many different ways. It provided us with the tools, not merely the hardware, but with a basic understanding, which for most of us, certainly for me, came absolutely for the first time of what you have to do to detect a signal in noise. So in all future experiments, whether by radio astronomers or microwave spectroscopists, people knew how to deal with the problem of the ultimate sensitivity of their apparatus and what you have to do to detect a weaker line and so on. That was all essentially worked out. One of the key people there, in the later years, was Bob Pound. And Bob's crucial contribution in our first nuclear magnetic resonance

work with Torrey and myself was his understanding of amplifier noise. Young as he was, he was as good as there was at the practical business of noise figures and inputs and receivers, which he had been working on under Zacharias. So we came out of that with that tremendously useful equipment, but we had to rethink values back into physics to see what physics would be worth doing. Now, of course, I'm sure the same thing can be said for the people who worked together in the Manhattan District on bomb physics or whatever, because there also was a tremendous advance; and they were directly in physics, so they didn't even have to figure out what to apply it to. What they were doing was physics.

**Sopka:**

Was your electrical engineering background from under graduate days of value to you in this …?

**Purcell:**

To some extent. It had already been of value to me as a research physicist because I was doing practical work in making magnets and things and running D.C. generators. In fact, it was probably of most use to me when I was working on the cyclotron, because

my job was essentially an engineering job. But it was useful in my work at Radiation Lab, although looking back on it, I didn't learn anything at Purdue that was directly applicable in the Radiation Lab because I didn't learn about electromagnetic waves at Purdue.

**Sopka:**

Wave guidance and terms like that probably weren't even in use then.

**Purcell:**

That's right. I often tell my freshman students, when I get Maxwell's Equation on the board for them, "I got a degree with honors in electrical engineering and I'd never seen these equations written down. Not only that, but the fact stated in the fourth equation was unknown to me, the fact that the curl of B is equal to E dot. We didn't need that at Purdue in 1932 because we never considered radiation problems. We only did waves on telephone lines. You do that all with telegrapher's equation. You don't need to know about displacement current. So I literally graduated in electrical engineering without even knowing there was a displacement current. But by

the end of the Radiation Lab I was very familiar with the displacement current." We learned about circuits in a general way. Bob Dicke was one of the leaders in developing the microwaves circuit theory in general terms, the scattering matrix and the impedence matrix. There were many things we didn't have, of course. We didn't have nonreciprocal devices. One of the things we would have dearly loved to have in Radiation Lab was a pipe in which the signal could go through one way and not the other. But the possibility of that had to wait for another several years until circulators were discovered, gyrators. And of course we didn't have transistors or anything like that. Everything was built with vacuum tubes. Well, that's how NMR[3] started, with that idea which, as I say, I can trace back to all those indirect influences of talking with Rabi, Ramsey and Zacharias, thinking about what we should do next. And at the end of Radiation Lab, when our thoughts were turning back to physics, Rabi had the bright idea of having some lectures to sort of rehabilitate us in physics. So he got Pauli. Pauli was then at the Institute. He got Pauli to come up every other week and give a lecture. By then the Radiation Lab was enormous, you know, thousands

of people, and many of them were physicists or people who wanted to be physicists. At the first lecture, an enormous lecture room was packed with people to hear Pauli. It was almost totally un-understandable to most people, including me. At Pauli's next lecture, attendance had shrunk by about a factor of four, as we still didn't get much out of it. But then Rabi had the great idea to get Julian Schwinger to lecture in the weeks in between. So Julian was there in the theoretical group — and, I don't know, — was all of 27 years old or something. Julian began to lecture. And that was just marvelous. He reviewed the recent developments, pre-war developments in physics, going back to the ‘30s and coming on — where things had gone, what were the puzzles, and that was just marvelous. So gradually the attendance at Julian's lecture went up and Pauli's went down. I remember the lecture when Julian was talking about the quadrupole moment as the deuteron and all that it implied and how you measured it. That was really exciting — the quadropole moment having just been determined by Ramsey and Rabi and Kellogg and Zacharias just before the outbreak of the war. But that helped us to get back into real physics. We wanted to do

something more than merely apply the tricks that we had learned.

**Sopka:**

You came back up the river then in early 1946, back to Harvard?

**Purcell:**

Yes.Well, see, we actually did the first NMR experiment here, not at MIT. But I wasn't officially back. In fact, I went around MIT trying to borrow a magnet from somebody, a big magnet, get access to a big magnet so we could try it there and I didn't have any luck. So I came back and talked to Curry Street, and he invited us to use his big old cosmic ray magnet which was out in the shed. So I didn't ask anybody else's permission. I came back and got the shop to make us some new pole pieces, and we borrowed some stuff here and there. We borrowed our signal generator from the Psycho Acoustic Lab that Smitty Stevens had. I don't know that it ever got back to him. And some of the apparatus was made in the Radiation Lab shops. Bob Pound got the cavity made down there. They didn't have much to do — things were kind of closing up — and so we

bootlegged a cavity down there. And we did the experiment right here on nights and week-ends. We were still working down there.

**Sopka:**

This was after the war had ended in August?

**Purcell:**

This was December, 1945.

**Sopka:**

But then there was a certain period of winding up.

**Purcell:**

Yes, and we were still working. We were working on the book, writing the books — all three of us: Torrey and Bob Pound and myself were among those people. And so we did the experiment here, but we were still employees of Radiation Lab. It was a mixed up thing.

**Sopka:**

Had Harvard already approached you about returning? I notice that you came back as an

associate professor, having spent your faculty years on leave.

**Purcell:**

That's right. All I remember — it must have been the summer of '45, something like that. I remember Ted Hunt and somebody — who else it was I can't remember — coming down to see me at lunch. We had lunch in a little hamburger joint down around MIT, and they said they'd been authorized to come down and ask me if I was interested in coming back as an associate professor, to which I said yes, and that was all there was to it. There was no haggling — nothing — and that's all I know. That must have been '45, I should think.

**Sopka:**

And your home base had been Cambridge really since you came here as a graduate student in '34?

**Purcell:**

Yes. We are living in the same house we were living in in 1940 when I was at the Radiation Lab, and we've been renting it now for 37 years and I'm just about to buy it.

**Sopka:**

That seems like something of a record.

**Purcell:**

It's the house that Roger Hickman lived in just before us, and they moved out to Belmont in 1940 and Beth and I moved in then and we rented the house from an old gentleman who wasn't so old then who was a physics teacher and professor at Northeastern, Mr. Coolidge. He died in January this past year, and we're about to buy the house from his estate.

**Sopka:**

I see — he held onto it.

**Purcell:**

He wouldn't sell it before. I tried to buy it.

**Sopka:**

So you can soon call your home your own.

**Purcell:**

We soon can call our home our own. So this stretch of Mass. Avenue I know. But during the war years I scarcely ever stopped off at Harvard. I mean there were years there where for six months at a time I never set foot in Harvard. I would just get on the subway and go down to MIT.

**Sopka:**

I guess that the atmosphere here was entirely different then.

**Purcell:**

Yes, it was full of the training programs. But then, of course, there was the Radio Research Lab over here, which was working on radar countermeasures. And they were allowed to visit us, but we weren't allowed to visit them. It was an unsymmetrical relationship with respect to security. Whipple and Felix Bloch were over there and Van Vleck was over there and Fred Terman ran it. But we didn't have much contact with them. The Radio Research Lab was recruited largely from engineers, not physicists. As witness the fact that in the Presidential election of 1944 — a

straw vote at Radiation Lab was overwhelmingly Democratic, at Radio Research Lab overwhelmingly Republican.

**Sopka:**

That's interesting.

**Purcell:**

Somebody I knew up here said, "We found one guy who voted for Roosevelt up here. He was the janitor."

**Sopka:**

Well, I gather that the Harvard atmosphere in the post-war years was very different and quite exciting in terms of picking up your physics research and in terms of the influx of graduate students primarily but students at all levels following the wartime experience.

**Purcell:**

Yes. We had those years when we had really very exciting courses with the older returnees in them who were very serious. The course that Chaffee gave

for so many years, I gave after the war a couple of times, Physics 28 we called it.

**Sopka:**

Yes, I took that with you.

**Purcell:**

You took that. And, gee, that was an exciting class. Those classes were full, as you remember…

**Sopka:**

They certainly were well attended. I mean they were large groups and they were really earnest.

**Purcell:**

Everybody was very serious, yes, really working.

**Sopka:**

But it was about then that the now Professor Bloembergen appeared on the scene and became involved with you.

**Purcell:**

Yes, he appeared in the spring of 1946 and I took him on as a research assistant, and he started right in producing. Of course, as you know, he got his degree back at Leyden.

**Sopka:**

Yes, he told me that on thinking about the idea of going through all the regulations here when he was already within sight over there, it seemed sensible for him to do it.

**Purcell:**

Right. No, those were great times. We had some wonderful students in that period — George Pake and Charlie Slichter, among others.

**Sopka:**

Do you recall how the evolution of ideas and then the realization in the laboratory went with regard to the development of the nuclear magnetic resonance work and the nuclear relaxation …?

**Purcell:**

Oh, yes, I remember. We weren't prepared for one aspect… Well, we realized that the crucial aspect in the experiment was the relaxation problem, whether we could get the nuclear spin system relaxed. And at the time we did the experiment, there was only one mechanism for relaxation that had been theoretically studied. That was in a paper by Waller in the late ‘30s, Ivar Waller, the Swedish theoretical physicist, which was written to analyze electronic spin relaxation in crystals and was applicable to the problem. It later turned out that it was not the primary mechanism. But at any rate Henry Torrey made a calculation with the Waller theory to see what the relaxation time would be, and we decided that although it looked as though, according to this theory it would be quite long, that at least it couldn't be much longer than that because this process had to work. So that our experiment was designed to take account of the possibility that the relaxation time might be many hours. Our first experiment was done on paraffin, which I bought up the street at the First National store between here and our house. For paraffin we thought we might have to deal with a relaxation time as long as several hours, and we

were prepared to detect it with a signal which was sufficiently weak so that we would not upset the spin temperature while applying the r-f field. And, in fact, in the final time when the experiment was successful, I had been over here all night — I can't remember; it must have been all day — nursing the magnet generator along so as to keep the field on for many hours, that being in our view a possible prerequisite for seeing the resonances. Now, it turned out later that in paraffin the relaxation time is actually $10^{-4}$ seconds. So I had the magnet on exactly $10^{8}$ times longer than necessary! The approach of Bloch and Hansen to the same problem was quite different, although we of course didn't know it at the time because we didn't even know they were doing anything. They did their first experiment on water and bubbled oxygen through the water in order to promote relaxation. That also was unnecessary, although perhaps not by such a big factor. Interestingly, we had approached the problem … mainly in terms of inducing transitions between two quantum mechanical levels. Bloch and Hansen and Packard apparently thought about the problem much more in terms of the precession of the classical spin moment. And, of course, these two descriptions are

totally equivalent. Nevertheless, I remember that the first actual personal contact between our two groups came when Bill Hansen came east a couple of months later — he frequently came to the Radiation Lab anyway — and we talked to him. We were talking at cross purposes for about 15 minutes before each of us understood the other's experiment in his own terms.

**Sopka:**

When was this, around 1945, '46? Was it that early?

**Purcell:**

Our experiment was done just before Christmas, 1945. And Bloch and Hansen's was done a month or so later. And the time I speak of must have been perhaps March, '46, something like that. Although Bloch had been at Radio Research, I saw very little of him during the war years; and we, of course, didn't know they were doing it — they didn't know we were doing it. And, as I say, we conceived of the thing in quite different terms. But the thing that we did not understand and it gradually dawned on us later was really the basic message in the paper that was part of Bloembergen's thesis and came to be

known as BPP (Bloembergen, Purcell and Pound), was the important, dominant role of molecular motion in nuclear spin relaxation, and also its role in line narrowing. So that after that was cleared up, then one understood the physics of spin relaxation and understood why we were getting lines that were really very narrow, which of course eventually became very narrow indeed when the high resolution NMR came in.

**Sopka:**

At what point along this evolution did the concept of the negative absolute temperatures come into the picture?

**Purcell:**

I can't remember the exact date, but it came in a little later than that.

**Sopka:**

Did that concept pose a hurdle or a barrier?

**Purcell:**

Well, it didn't bother us. In fact, I regarded it at first as just a mere pedagogical point. It came at the time

when Bob Pound was doing a lot of his work on nuclear quadropole resonance in crystals. I had been thinking a little about the spin temperature and had just noted with amusement that if I had inverted levels, I could describe it with a negative temperature, that the thermodynamic relation between entropy ds, and dq/t was perfectly valid for negative temperature, and that everything would go through formally with no particular trouble. And in fact I had started to write it up as a communication for the AMERICAN JOURNAL OF PHYSICS, thinking of it purely as a kind of an amusing pedagogical point. Then Bob came up one day with this crystal that had a five-minute relaxation time, the longest spin-lattice relaxation time we'd ever seen. So I said, "Look, Bob, if we've got this, why don't we just do it? Just for fun let's invert the spins and show that they behave as if they were at negative temperature." So we devised an experiment to do that and it behaved just the way we thought it would. And so then people began taking it a little more seriously, and when we began talking about it, we found that it really bothered people. Well, we wrote up our little letter about the spins in the crystal sample and pointed out that it could be said to be at a

negative temperature. And then some people, especially chemists, were terribly bothered by that — old-line thermodynamicists.

**Sopka:**

I had heard that. That was why I was wondering whether it bothered you people or not.

**Purcell:**

No, it didn't bother us at all, but I didn't want to get into any arguments … Fortunately, I didn't have to because Norman Ramsey shortly thereafter spent a sabbatical year at Oxford. Simon, the great thermodynamic physicist at Oxford, had been greatly intrigued by the negative temperature idea from the start and was not bothered by it. He enjoyed it very much, and he kept urging Norman to write it up. So Norman, while he was at Oxford, started writing an article on negative temperature which he published. Then Norman began getting these really stern and indignant letters from Giauque at Berkeley. Norman showed me one of these letters from Giauque one time. The only conclusion you could draw was that Giauque just plain didn't believe in statistical mechanics. You know, he was an old

thermodynamicist. Well, there are indeed difficult questions that can be raised about the concept. The whole thing — I must say, if we had tried to deal with it seriously then, we would have had to face some questions that I think we would have had some trouble with: namely, what is the state of the system when it's demagnetized, the spin system? And later on, a number of people, most especially Anatole Abragam, contributed to the clearing up of that issue.

**Sopka:**

At what point did it become clear that this had some relevance to the question of masers? Do you remember how that came about?

**Purcell:**

Well, the idea of the inverted population was of course basic to it and that when you had the population inverted, it was an emitter, not an absorber. This whole sort of complex of ideas were knocking around. I would put in there Dicke's idea of the super-radiant state.

**Sopka:**

It's interesting to think about what ideas were floating around in the active scientific population.

**Purcell:**

That's right. It was the general idea I would say of people thinking about what happens if I have an inverted population so that I have more in the upper state than in the lower state. How does the radiation interact? I have stimulated emission that exceeds the absorption, put it that way. Now, if I make the stimulated emission exceed the absorption and then use it to make itself a bootstrap, I have a laser. This super-radiant state idea of Dicke's is a slightly different situation, but all these ideas are aspects of a situation where we've suddenly moved out of the conventional realm of upper states always having lower populations than lower states.

**Sopka:**

The kinds of things you thought of when you took Oldenberg's course.

**Purcell:**

That's right. Here things are backwards. Here things really can happen the other way around. So I would regard our introduction of negative temperature as just one of the ideas and papers opening the door on that whole world.

**Sopka:**

This post-war research work that you did in the late ‘40s, was that financed under this naval office or was it locally financed?

**Purcell:**

No, at some point — I've now forgotten just what the years were — I had a grant from ONR. [4] We were working under an ONR contract. In fact, we had a sort of blanket nuclear physics contract under which this could go, so I didn't have to spend much time myself getting a separate contract or anything. That was what was so nice, because we had the support we needed: we didn't have any strings; we didn't have to have a lot of individual contracts. They were quite willing to see nuclear physics interpreted broadly. Other things I did were not so

supported. For example, the 21 centimeter work, which came shortly thereafter, that was done on our own funds and a tiny grant from the American Academy: $500 I think we got from the Academy, which we used to make our plywood antenna, and the rest of the stuff we borrowed…

**Sopka:**

How did you get into the 21 centimeter line work? I realize that the young man, Harold Ewen, that you worked with was at least partly, if not basically, an astronomer. Or wasn't he? His degree was in astronomy and physics?

**Purcell:**

No, no, his degree was in physics, and he had an interest in astronomy, but he came to me for a thesis topic. Ewen originally thought he might be able to use microwaves somehow to detect or measure something in the upper atmosphere. He had been in the Navy during the war and had been involved with a lot of technical things. He was already technically pretty adept. And he was interested in astronomy and knew some astronomy because in fact he'd been teaching, among other things, celestial navigation to

naval air cadets during the war, including Ted Williams. Well, we talked about the upper atmosphere prospects. We didn't see anything particularly exciting and we were still talking about it when Ewen went off to an astronomy meeting somewhere. When he came back he said, "You know, somebody was talking about this hydrogen hyperfine line, whether you could detect it." So we began thinking about that and just looking to see what were the sensitivity problems, whether we couldn't… and decided maybe there was a fighting chance to do it. So he took that on as his thesis topic. Now, at the time, to the best of my memory, we did not know the origin of the suggestion — namely, van de Hulst. I am quite sure, though, that the chain going back ends at van de Hulst. He was a young Dutch astrophysicist at Leiden who had made the original suggestion that one should look for a 21 centimeter line, and I feel sure that it's an echo of that suggestion that Doc heard at the meeting that put us onto it, although at the time we didn't know about that. We were fairly well along having decided to do it and making our plans, when we made actual connection with van de Hulst.

**Sopka:**

This was in the early '50s?

**Purcell:**

Yes, the early '50s, that's right. The first observation was in the spring of '51.

**Sopka:**

I see your trophy.

**Purcell:**

Yes, the boys made that for me last year to commemorate the 25th anniversary. As it happened, the actual time Doc made the observation van de Hulst was here as visiting professor at Harvard.

**Sopka:**

Oh, that was a coincidence.

**Purcell:**

And came over and we talked to him about it and everything, and he then cabled back to Leyden where they also had been getting ready to look for it. They were a little behind us. But I have never

doubted that the credit for the original suggestion that one should look for 21 centimeters must go to van de Hulst, because I don't think… I know of no shadow of a doubt on his claim to have that priority. Doc had great nerve to look for it though, and he did a superb job of getting the stuff together. I told him that it was going to be a very tough thesis problem because, "if you don't find the line, you're going to have to put in a hell of a lot of work to establish the limits. If you find it, of course" — as he did luckily — "then it's great, it's fine. But if you don't detect it…" And of course the original detection was such that if it had been even five times weaker, he probably wouldn't have seen it. It was just barely there.

**Sopka:**

There were fortunate circumstances all around.

**Purcell:**

Yes. But on the other hand, we claim we went at it right. We used the simplest possible antenna but one which was good for the job. We couldn't have done much better with a larger antenna. And Doc's receiver was really very good. It was probably as

good a receiver of that type as existed in the world at the time he did the experiment. It was superior in one important respect to the receiver at Leyden.

**Sopka:**

Since van de Hulst was here, did he discuss with you what approach and what difficulties they had been pursuing in Leyden?

**Purcell:**

No, but we knew a little bit about it. They were making a bigger antenna. They were going at it in a more elaborate way, which eventually enabled them to get better results when they finally got it. They had had the misfortune of a fire in the antenna, which set them back a week or so. So, in fact, it was I forget how long after I had asked … The original publication consists of three communications in NATURE of the same adjacent pages,[5] because I asked NATURE to hold ours up until they heard from Leyden so we could publish together. And at the same time I wrote Pawsey in Australia and the Australians had not thought of looking for this line, didn't know anything about it, but they were so expert in the radio astronomy business and had so

many things going that it didn't take them very long to throw together a 21 centimeter receiver for one of their antennas.

**Sopka:**

This was just when the whole field of radio astronomy was really bursting …

**Purcell:**

That's right, bursting open, and the Australians were very active. So then Pawsey cabled in that they had confirmed our results, and that cable is also with the two pieces in NATURE in that first publication: the letter from Doc and me and one from Oort and Muller at Leyden and a cable from Pawsey. I've always been glad that I handled it that way.

**Sopka:**

It certainly seems like a very gracious way…

**Purcell:**

Well, if we hadn't done it that way, there would have been long-standing rancor. There probably is even some residue of that, a little bit.

**Sopka:**

Was this immediately recognized for its later potential as a tool, or did it take a while for this to…?

**Purcell:**

No. It was certainly apparent to Oort right away. You see, Dort is the grand old man of Galactic astrophysics, and right away he saw it as a tool for learning something about the structure of the Galaxy. I don't think they recognized perhaps immediately the trick of using the Doppler shift to locate the hydrogen they were looking at, but that came very soon. So there was no question but what… No, even in my mind, knowing as little astronomy as I did, it was clear that once you could do this, you could learn a lot.

**Sopka:**

I know that in recent years you've become increasingly interested in problems in astronomy and astrophysics. Did this all stem from this period?

**Purcell:**

Well, pretty much. That was certainly the first time I'd had anything personally to do with astronomy.

**Sopka:**

Did you have a course in astronomy?

**Purcell:**

Oh, no, no. I couldn't find any stars or anything like that. Now, that's not true of Doc Ewen. I call him "Doc" because everybody called him Doc even before he had his Ph.D. It doesn't have anything to do with that. In fact, he had to tell me where the Galactic ordinates were and everything.

**Sopka:**

I looked him up in AMERICAN MEN OF SCIENCE. After he did this work, then he went in business for himself, did he, and he's still in this general part of the country.

**Purcell:**

That's right.

**Sopka:**

He lives in Natick or Weston or someplace like that.

**Purcell:**

That's right. He has a small company called Ewen-Knight Associates, and for a while they were in the business of making very advanced electronics, including radio astronomy receivers, and various military receivers and things. In recent years his company has gotten out of hardware production and he's mostly doing consulting now. He’s a very interesting fellow. He was sort of an entrepreneurial type, extremely good at getting...

**Sopka:**

He was among the older group of people who came through after the war?

**Purcell:**

Yes. Well…

**Sopka:**

You said that he had taught...

**Purcell:**

Yes, but he'd been pretty young at that point. I don't know. I don't know. I don't think he was more than in his late twenties when he did this.

**Sopka:**

I see. He wasn't somebody who'd had a large interruption of his life.

**Purcell:**

No, I don't think so. As it happened, various members of the Boston Red Sox were friends of his who had been in the Naval and Marine Air Force: Johnny Pesky and Ted Williams. Ted Williams once visited the lab here and came over with Doc. Everybody was all aflutter.

**Sopka:**

That was back when the Red Sox were going strong.

**Purcell:**

Yes, I remember the year Ted batted .400, one of the years.

**Sopka:**

I noticed that you've had 18 Ph.D. students.

**Purcell:**

Is that right? I never counted them.

**Sopka:**

And all but two of them were before 1960 and the ones since 1960 were joint, had other advisers in addition to yourself.

**Purcell:**

Oh, those were perhaps people that I really didn't… where I was just serving as a formality. Who are those?

**Sopka:**

I'm not sure. I didn't jot down the names. They're in that little book that the department published last year.

**Purcell:**

Well, I tell you, they were people like Pat Palmer, for example, who did his thesis in astrophysics — a

brilliant thesis working with Ed Lilly — and I was simply his physics department signer upper, so to speak. I was following what he did and had great interest in it, but I wasn't his supervisor. He's not my graduate student in any real sense.

**Sopka:**

Well, I was wondering how the evolution of your own research style went in the period from the '40s to the '50s to the '60s. On the surface it at least appears that by the '60s you preferred to work alone rather than have the responsibility of guiding people. Is that a valid conclusion to reach?

**Purcell:**

Well, yes. I guess the real thing is that after a long period with NMR stuff, I guess I just didn't have any more ideas for that that I wanted to work on; and I really have been one to develop a kind of coherent ongoing self-perpetuating program of research. So I was dabbling really in different things in kind of an opportunistic way, which is not particularly good for graduate students, and I just kind of slipped into the role of not taking graduate students. I'm not broadly enough trained certainly as an astronomer, so that if

I were to take a student on in astrophysics, he wouldn't be getting himself really connected up with a central part of the field. I've worked with a number of people, published with some people, who are other people's graduate students, but I haven't felt I really had any field as it were to offer a graduate student where you can come and do a thesis and then you'll have made a position for yourself in a field. I don't have any such field.

**Sopka:**

Your own interests seem to have continued to evolve and to range fairly broadly.

**Purcell:**

That's right and that's a luxury which I'm fortunate to be able to indulge, and the price of it is that you don't have graduate students carrying on the work. I'm not particularly proud of this way of doing business. In fact, I often regret that I haven't attempted to keep a central field going so that I'd have a base and have a continuing turnover of graduate students, as Norman, for example, has, to the great profit of the department. In that way I've really not contributed my share to the educational

processes around here, and it's a selfish way to behave in some respects. It's just my style. Now I'm interested in interstellar medium and in this biophysical stuff I'm doing, which is really becoming more interesting to me now. So as I look now ahead a little bit, I would see myself perhaps tapering out of the interstellar medium and into bacterial environment. All through the '50s and '60s, of course, I've done an awful lot of government consulting stuff and things of that sort that really took a good deal more time than is visible around here. I do not do that anymore at all.

# Interview Session – 3

**Sopka:**

It is now June 14th and I'm again meeting with Professor Purcell in his office in the Lyman Laboratory of Physics where we will continue the discussion that was begun last week on June 8th.

**Purcell:**

Perhaps I should say something now about my teaching career in the department and my teaching interests. Most of my teaching actually has been in undergraduate courses, and my involvement in particular in the freshman and sophomore courses goes back to my first years as a teaching assistant. I worked, as I think I mentioned before, in Professor Black's Physics B and later in Physics C under Professor Saunders. I think perhaps Professor Saunders' course was the first one I actually was responsible for, after he retired. Professor Chaffee's course in electron physics, which had been a tremendously interesting course for me as a graduate student, fell to my lot just after World War II and I had a great deal of pleasure in teaching that material,

most of which I'd learned first from Chaffee. One of the courses I introduced -- I think it must have been in the middle ‘50s more or less -- was a course in electromagnetic waves for undergraduates called 161, which drew heavily on my experience at the Radiation Lab in microwave circuits. A more recent innovation that I've introduced and tried a few times is a course called "Widely Applied Physics," which was conceived with the idea of introducing physics undergraduates to some other neighboring fields in the hope that some of them might find an interesting direction for their own careers and also I think to indulge my own taste for engineering physics, applied physics of an engineering sort, which has always remained strong. In fact, if I haven't said it before, I'll say now that I really by intellectual taste am almost as much of an engineer as a physicist. Much of what I've done in experimental physics, especially the work that came out of the Radiation Lab, was engineering in style, and I've always found that applications of physics in that field very interesting. I haven't taught many graduate courses. I've enjoyed particularly teaching introductory quantum mechanics and atomic physics. I've never taught advanced mechanics. In fact, I'm not even

sure I had a course in advanced mechanics. I did teach thermodynamics a couple of times, enjoyed that because it finally forced me to learn some things that I had really never had command of before. I've been interested … I'm still interested in undergraduate teaching, and I imagine in the concluding years of my membership in the department I shall teach probably only undergraduate courses.

**Sopka:**

I understand you have given courses under the general education umbrella.

**Purcell:**

Yes, I gave Natural Science 2 one semester, enjoyed it very much, and I'm sure that there are lots of good courses that can be made for undergraduates who are not physical science concentrators. These are very bright people we have at Harvard, and there are many ways to challenge their interest in science.

**Sopka:**

The University is in the process of reexamining its program in that area. Do you have any comment on

possible directions it will or should move in coming to grips with this group?

**Purcell:**

I think it's time that the enterprise was re-examined. One always gets a temporary benefit from re-examination at least. I'm not particularly impressed by the formula that the so-called task force on the core curriculum has proposed, but I'm willing to see what comes from it. I find the description of it and some of the justification, as explained in the core curriculum report, really does not express my conception of the university or of the capability of its students. I think it is rather patronizing to the students. Harvard University is a very special place. My ideal for Harvard University is simply a university where every course is a good course, but I don't think one can presume to define minimum requirements for an educated Harvard graduate in terms of a particular set of courses.

**Sopka:**

How do you feel about the idea that there should be special courses designed for people who are not concentrating in that field?

**Purcell:**

I think that's necessary, absolutely necessary.

**Sopka:**

It certainly seems to be in the sciences and the group of courses under the Nat. Sci. designation. Do you think it's also desirable in areas like art and music and sociology? For instance, your physics undergraduates, should they be taking a course designed for non-concentrators rather than taking a course that would be the introductory course in the department anyway?

**Purcell:**

Well, I'm inclined to think that it doesn't matter so much for the physics concentrators. I'd really rather see them take a departmental course in social relations or history or economics or whatever. The situation is not symmetrical. The other way around we have the problem of mathematical requirements among other things. And also we have a genuine duty, it seems to me, to try to exhibit those aspects of our science which have some usefulness and relevance to the career of a person outside science. I

think the physics concentrator can take a course in economics and follow it and decide perfectly well what part of it is relevant to his future concerns, if any. But I think it's too much to expect a student of economics or history or English to do the same for a physics course. Moreover, I think it's a very interesting and exciting and challenging job to look at a science that way and try to explain the ideas to people, and I would insist that what we ought to explain is the ideas and not just the language -- some of the ideas, some of the ideas, by no means all of them. Here we have to take maximum advantage of that wonderful property the human mind has of making a world from one good example. That's the thing that makes courses like Nat. Sci. 2 feasible, that you don't have to cover everything, as long as what you're covering has some important ideas in it.

**Sopka:**

Presumably you fell that it would be important for the members of the physics department in the future to continue to take responsibility for teaching this kind of course …

**Purcell:**

Oh, yes, and I think we have in the past. I think our record is really very good.

**Sopka:**

Yes, I do, too. I was thinking about the future.

**Purcell:**

Ted Kemble really created Nat. Sci. 2, too, and more than that, he really set the standards high so that it was a course worthy of Harvard University. Well, let me talk now about some other involvements at Harvard, which I don't plan to say very much about: my services as Senior Fellow in the Society of Fellows for nearly 20 years. I enjoyed this very much, and I have a tremendous admiration for the contribution that the Society of Fellows has made to Harvard University, the magnitude of which is obvious if you simply look at the present faculty and see how many of them were Junior Fellows. I was not myself a Junior Fellow, of course, but I knew some of the early Junior Fellows so that when I was invited to become a Senior Fellow -- I guess it was

around 1950 -- I had no hesitation in taking on that mild responsibility.

**Sopka:**

As a Senior Fellow, were you responsible for the choices as the years…?

**Purcell:**

Yes, that was, in fact, our only important duty: to interview the candidates and elect the new Junior Fellows each year. We interviewed every candidate. Every serious candidate was interviewed by all the Fellows, and I picked up a good deal of general education information listening to my colleagues in literature and history and so on interview these candidates, and also I learned even more from the Junior Fellows themselves over the years, with whom of course we dined every Monday night during term time, and I made very many friends there among the younger scientists and younger humanists. I resigned finally simply because it seemed to me 20 or 21 years of Monday night dinners was perhaps enough, but it was a very friendly and rewarding and pleasant association for me.

**Sopka:**

Do you think that this concept of the Society of Fellows, which certainly is of great value both to the individuals and to the University, could in any way -- the ideas of it -- be in any way extended to a larger group of people at Harvard or could be emulated at other institutions?

**Purcell:**

Well, it is already emulated at the University of Michigan. Michigan some years ago established essentially -- I don't think it would be too much to call it a carbon copy of the Harvard Society of Fellows, and it's running full blast as far as I know. It's fairly well endowed. The endowment of the Society of Fellows here did not over the years grow as much as it really ought to have grown. The Society was still operating on Mr. Lowell's original bequest pretty much. But it's flourishing, and I'm happy to say that it seems to me the Society of Fellows is as vigorous and as promising right now as it ever has been. There are sensational young people in it, including women now.

**Sopka:**

Yes, I interviewed Louise Dolan.

**Purcell:**

Yes, it's really one of the great things around here. It's just really an immeasurable asset to Harvard. Mr. Pusey really never understood that and never accepted that actually.

**Sopka:**

In saying that it's of inestimable value to Harvard, is this because it's the bringing together of a group of very bright young people and allowing them to cross-fertilize and then this seeps out into the university structure?

**Purcell:**

No, I was simply thinking of the fact that it's been the recruiting ground, as it were, for a fair fraction of the Harvard faculty. These are extremely bright people, very productive. They come to Harvard; some of them were of Harvard originally but many were not. If you just go down the present faculty of arts and sciences and check out any who were

former Junior Fellows … In fact, if you look at the Harvard administration now, even from Rossovsky; Dreben, who was Dean of the Graduate School; Frank Pipkin, who is Associate Dean; Harvey Brooks, who is Dean of the division; McGeorge Bundy, a dean some time back and just keep going… I mean these people you identify with Harvard scholarship and leadership very often turn out to have been Junior Fellows. It's really astonishing. We made a survey of that some years ago when we were trying to get some money for the Society, and I was astonished by the statistics. I think about half of the math department were Junior Fellows and Gleason, one of the most distinguished mathematicians we have, and several others over there.

**Sopka:**

Garrett Birkoff was one, wasn't he?

**Purcell:**

Yes, and Mumford, Lynn Loomis, lots of them. Well, let me go on now to this outside activity -- professional and activities outside the University, which in my case have involved a good deal of work

on one or another government scientific advisory committees or projects. The depth of my involvement in these things is really not apparent from the ordinary chronological record, because some of the heaviest involvement was in the form of summer projects or ad hoc projects set up to address a particular problem on which I would be working full time with a group of people for perhaps several months. Just to mention these in what I think is chronological order: I was involved first in a Navy project called the Hartwell Project run by Zacharias and concerned with the problem of anti-submarine warfare in 1950 at a time when snorkeling submarines represented the current threat. Shortly thereafter I was involved in a project for the State Department, which was concerned with the Voice of America broadcasts, which were our propaganda arm in Europe. The Voice of America -- the radio broadcasts were being jammed everywhere behind the Iron Curtain as a rule, although people were still managing to hear them now and then. And our project had a large technical section of which I was in charge, directed as the purely technical problem of how you could prevent the jamming or broadcast through the jamming, etc. We reported -- I think that

must have been ‘51, because we reported our findings to Dean Acheson, who was then Secretary of State under Truman. I think actually we weren't able to do much good. We didn't invent a very effective remedy for the jamming. We did, however, as an incidental spin-off, as we would now say, start what's known in radio engineering as scatter communication, ionospheric scatter communication. Some of the experiments we promoted led to the discovery that one could transmit radio waves over the ocean by scattering them off irregularities in the ionosphere. This became known as ionospheric scatter and had some modest utility in later years.

There's a scientific paper which came out of that with a large number of authors concerned with those experiments. Sometime after that -- I think it must have been in '52, perhaps the spring of 1952 -- I was involved in (or perhaps the fall of '52) a project called Beacon Hill, which was a study of the possibilities of reconnaissance, photographic reconnaissance. This was almost entirely a technical study. People actually worked in Boston for several months. The conclusions of this study were the basis for much of the activity in later years in the developments of photographic reconnaissance both

from airplanes and from satellites, and in that sense it marks the beginning of my own connection with projects of that kind. Then in 1954 I was recruited into the rather broad study of the United States defense capabilities, technical capabilities, posture, under Dr. Killian -- the so-called Technological Capabilities Panel, to give it its cover name -- and in this work I was particularly involved in a small group concerned with the technical problems of intelligence gathering.

**Sopka:**

This was a period when the scientific community was cooperating quite actively with the governmental groups.

**Purcell:**

Yes.

**Sopka:**

Apparently primarily because of the real concern of everybody for the tense …

**Purcell:**

The cold war was on. And I don't recall any… well, there may have been some, but you're quite right in saying that by and large the scientific community was either passive and simply didn't take part in this at all or else they were contributing. Those of us who were involved in these projects were ones who had been more or less continually involved since World War II in military technology and things like that. So it was natural for us to keep in the game.

**Sopka:**

There was no trouble continuing or extending the working relationship between yourselves and the military and the political leaders at that time?

**Purcell:**

No, none at all, and this of course … by now we're in the Eisenhower Administration. Killian had extremely good relations with Eisenhower. In fact, a number of the people knew Eisenhower and were trusted by him. We had direct access to the President on any really important matter when decisions had to be made. There was no holding back. The problems

were very difficult. Many of them were not solved in any real sense, but there was very active cooperation and exchange of views.

**Sopka:**

And presumably the military and political leaders were very desirous of any help they could get from the scientific community.

**Purcell:**

Yes, the people that we were dealing with were people who had gotten such help during World War II and knew how to deal with the scientists. There were many issues that were … well, I don't want to get into this because it's really not part of my activity, either before or since, but you could see the shadow of things ahead in some controversies at that time. I think particularly of the question of the nuclear powered airplane, ANP, nuclear powered aircraft. It was almost the beginning-not exactly … of the hawk-dove split, but the military in the nuclear industry were very determined to make an airplane that was nuclear powered, and every scientific study of the thing showed that it was a bad idea. And yet it couldn't be killed. You could never

kill the thing because G.E. or somebody would always revive it. So for about ten years the nuclear powered aircraft was a project which most of us thought was stupid but we couldn't stop. Finally it was canceled, and it would have been stupid. The stupidity grew and grew. But that was the kind of an issue that was typical of ones we have now, very much like the B-1 bomber or the SST and so on. It was perhaps the first sort of example of that kind of thing.

**Sopka:**

It's interesting to know that because I was trying to get at when the relations began to cool, as it were.

**Purcell:**

Yes, that was a slight rift in the lute, let's say. The ANP it was called if you ever have to look it up: Aircraft Nuclear Propelled. This terrible object would have flown at subsonic speed and would have been a sitting duck and was not good for anything. The Killian Committee resulted really in the formation of the new President's Science Advisory Committee, and I became involved in that when it formed under Eisenhower. And just about that time

we had Sputnik and the dawn of the space age and the pressing questions of policy as to how the United States was going to organize itself to develop space technology and satellites. I was involved in a number of other problems in PSAC, but perhaps the chief one I was involved in was the question of the formation of NASA[1] -- the question of whether the development of space technology should be given to the military or should be the province of a newly formed totally new agency imitating the history of the AEC, or whether, as actually turned out, one should expand the NACA[2] and give it the task of space. There were strenuous arguments on all sides of this question. The future of space science was dimly foreseen, but in retrospect after Sputnik, and after a lot of discussion, and of course thanks to the quite considerable progress that had already been made in rocket technology and the military, the main outlines of the next 20 years in space really became fairly clear.

**Sopka:**

Was Sputnik really the fuse that it's been considered to be in terms of igniting our space program?

**Purcell:**

Well, certainly it was very important. There were other things, though, that aren't quite so well known -- namely, the development of intercontinental rockets and so on. There's a recent paper by Herb York*and one of his students which addresses exactly this point: what were the various developments to which the present status of space and space technology can be traced, and you really ought to look at that to get it straight. You see, there was the decision to try to develop long-range rockets that followed a study by von Neumann and Wiesner -- I think it's sometimes referred to as the von Neumann Committee -- a classified study of that or the possibilities there. We were concerned at the time with the open aspects of space science and making sure that people understood the possibilities that could be opened up by satellites. We prepared within PSAC, a few of us, what we called a Space Primer. This was to be issued by the President for everybody to read, and it explained what satellites do and what we can expect from them and so on. Two or three of us worked on this and in particular Edwin Land and myself -- perhaps one or two others, but Land and I wrote most of it with the help

of Frank Bello, who was then associate editor of FORTUNE magazine and has been for many years associate editor of SCIENTIFIC AMERICAN. [3] I think our little Space Primer really stands up very well.

**Sopka:**

Was that published as a government publication?

**Purcell:**

Yes, government publication, and I think I still have one. It's tucked away somewhere. We talked about eventually going to the moon. Our time table wasn't too far off, although it was a little more expensive than what we said. We talked about weather satellites and what that might do and a number of things like that, which have all come true. The people that needed to be educated were not merely the public at large but people in responsible positions in Washington.

**Sopka:**

Who was the President at that time?

**Purcell:**

Eisenhower. Herb York and I had a little talk that we prepared that we gave jointly, equipped with some charts for the easel and so on, and we went around Washington giving this little tutorial lecture on space. We gave it twice in the Cabinet Room, once to the President and the Cabinet and once to the President and the Security Council. Eisenhower is one of the people I've given the same lecture to twice. We went over and gave it to the State Department and went around various other places to tell these fellows about space. I think what we told them turned out to be really true.

**Sopka:**

How did they respond at the time? Were they, in other words, ready to accept this broader picture rather than just military capability?

**Purcell:**

Yes. They responded like good students. They were interested. Each was interested in his own way. I recall that the chairman of the Security Council at the time was a fellow named Bobby Cutler, who was

a Boston lawyer. And he kept insisting that we give him a legal definition of outer space. It was clear that we were heading into a time when the notion of what a country had jurisdiction over would be very difficult to deal with. In fact, as we pointed out, if you imagine the country's boundaries going out from the center of the earth out through the country in a cone, you see -- if you go far enough out, the cone is moving with the speed of light. So you can't stay within the United States without moving faster than the speed of light! But in fact this foreshadowed a very important question that I was greatly concerned with later -- namely, the question of the freedom of space, the freedom of the skies, for satellite reconnaissance and indeed for airplane reconnaissance, which was to confront us in such a painful way in the U-2 development. And then later on to confront us, but really be resolved, by the course of events in satellite reconnaissance. So alongside of these activities, I was involved in many of the technical developments or at least keeping track of many of the technical developments in photo reconnaissance from aircraft and from satellites. The U-2 project was really an outgrowth of the studies we made in the Killian Committee.

The eventual program in satellite reconnaissance, now a very active operation in both major powers, grew out of that. And the question of freedom of space really got resolved, more or less resolved, simply by going ahead and making the assumption that space was open to all. Really the only way one could have met Cutler's demand for a legal definition of outer space was to say that outer space is free, but then you have to say when is it outer.

**Sopka:**

Something like the 12-mile limit.

**Purcell:**

Yes. Well, it's easier because you can say that if the thing is in orbit, that's outer space. Well, this was just to explain some of my own involvements in Washington over the years beginning in 1950 to 1970, roughly speaking.

**Sopka:**

Did this require your spending considerable time away from Cambridge? Did you have to spend much time down in Washington?

**Purcell:**

I traveled to Washington a great deal during that time. It was all in cut-up pieces of two or three days at a time.

**Sopka:**

I see. You didn't have to pick up and go and settle some place for three or six months.

**Purcell:**

No, that only occurred in the case of these summer studies that I referred to.

**Sopka:**

You were a member of the President's Science Advisory Committee in two different intervals. During the second one, the '62 to '65 -that would have been under President Kennedy and then President Johnson…

**Purcell:**

Yes. Well, I went back on the committee when Jerry Wiesner went in as Kennedy's adviser, not immediately because Kennedy, after all, became

President in '61. Then I can't remember when that period of membership in PSAC ended. Does it say in your list? What does it say there?

**Sopka:**

It says the first one was 1957 to '60, and the second…

**Purcell:**

Was ‘62 to ‘65, yes, and so two years under Johnson.

**Sopka:**

I was wondering whether there were any differences in the political and personal climate between those two periods.

**Purcell:**

Oh, yes.

**Sopka:**

In terms of whether you felt serving on such a committee was a suitable and effective role for you to be playing?

**Purcell:**

Oh, yes, I felt it was at that time. In fact, in ‘64 when Johnson was elected on his own, we were quite enthusiastic for his election. I remember being. Of course, he was running against Goldwater. There was already, of course, a polarization in the physics community dating from ‘54, from the Oppenheimer case and all that. And then with much more substance … Well, a polarization which also involved the question of arms limitations: the hawks vs. the doves as the way to characterize it. Let's say Teller, the Berkeley-Los Alamos, the whole Teller axis against the scientists that wanted to proceed toward arms control, which Teller was against basically. And that was really a very strong polarization, and we saw that all through the … You can see that very clearly in the latter part of the Eisenhower Administration in George Kistiakowsky's book.

**Sopka:**

His recent DIARY OF A SCIENCE ADVISOR?

**Purcell:**

Yes, that tells you a great deal. You see, I was on the committee under George during all those years, and that book is a very accurate reflection of what went on. It's just a diary so that it's not a continuous narrative, and lots of things are left out, but it gives a very clear picture of the struggle. The real struggle there was between the hard liners who didn't want any arms limitations at all and the whole test ban treaty, you see, with the idea of stopping nuclear testing: a very important period there. I was involved in that somewhat but not nearly as centrally as were the people who were more directly involved with atomic weapons, but I saw a little bit of it. And that was the polarization then which of course grew somewhat more broad and more plain when the Vietnam War finally came and really … I continued to be on a PSAC panel, as we called it, into the Nixon Administration. The panel that was concerned with reconnaissance, although I was not a member of PSAC. We would have a special panel on some subject and it would have scientists who were not members of PSAC but who were experts in that particular subject.

**Sopka:**

Did the final date of ‘65 have any particular significance for the ending of your formal serving on the PSAC Committee?

**Purcell:**

I think we had a term. I think by then my term simply came to an end in ‘65. I didn’t sever all my connections with PSAC until the middle of the Vietnam War when I finally wrote a letter just resigning flatly from everything. I'm ashamed to say how late it was. I think it wasn't until maybe the bombing of Laos, and then I just withdrew from all government connections. Anything connected with the White House I resigned from.

**Sopka:**

You were presumably specifically disenchanted with what was going on at that time. Would that extend in general to your feeling about scientists and yourself in particular working with the government operations?

**Purcell:**

No, not if the government's doing something sensible, but here the government was engaged in a crime, which we were all a little slow in recognizing. Or at least I was slow in recognizing how bad it was.

**Sopka:**

At the present time are you involved in any such governmental consulting?

**Purcell:**

No, only by way of the National Academy, to the extent that the Academy performs studies. I am on a committee or have been on a committee that reviews Academy reports on controversial issues, and I've just finished reviewing one. But I'm responsible there to the Academy …

**Sopka:**

And not to any particular administration?

**Purcell:**

… and not to any particular administration. No, I have no connections at all with the present administration nor the previous one.

**Sopka:**

Could we turn for a moment to your work within the American Physical Society? I notice that you were president during one period, and of course you must have been involved in this matter of the split within the physics community that you mentioned before, about the doves and the hawks. Do you feel that the political and military developments have had a great impact on life within the physics community in terms of cordiality between its members and the difficulties of a president of a physical society at that time when …

**Purcell:**

Well, the split between the nuclear weapons doves and hawks the hawks vs. the arms limitation people -- was not really echoed as such within the Physical Society. The number of physicists who were actively supporting Teller's views, just to use that as a way of

defining it, was really a very small fraction. These were the weapons physicists by and large and not all of them even. The conflict in the Society during my term of vice-president elect, etc., etc. -- two or three years there when I was actively involved -- the conflict in the Society was basically a division between those who thought the Society should be changing in the direction of taking an active role in public affairs and taking care of its members and taking positions on public questions and those who thought it should stay as strictly a society devoted to papers in physics, physics research as such. I suppose if one could characterize it by extremes, the most extreme on each side, you might pick Karl Darrow and Charlie Schwartz as the opposite ends of the spectrum.

**Sopka:**

Would you care to comment on your own view?

**Purcell:**

Also at that time the thing was exacerbated by the general ferment in the universities and so on and the upheaval of ‘68, ‘69 and ‘70 in the universities, which set the pattern for some of the agitation within

the Society. I think in those years the Society managed pretty well not merely to balance off so that one ended up with a Society as a whole more or less in between, but actually to start some constructive evolution of the Society in some wholesome new directions that it seemed to me at least when I was an officer that we couldn't turn our backs on some of the changes that were desired. And I must say the members of the Society responded extremely well. I found the two years I was closely involved rather agonizing because it was not something I like to do, or do well, but offsetting that was the really quick and generous response of anybody in the Society that I asked to help. I mean, it was really amazing how people responded. So that we organized the Forum, and just by a lot of telephone calls we recruited people to take an interest in that, and these were people whose views ranged not quite as wide as the spectrum between Charlie Schwartz and Karl Darrow but pretty widely, and they worked together. The present complexion of the Society, I think, is simply the further evolution of that.

**Sopka:**

You feel then that it weathered the storms of the late ‘60s and early ‘70s. Do you feel it's now moving in directions that you would approve of?

**Purcell:**

I think it's moving in directions that it really had to turn to. It wasn't really a question of weathering the storm and coming out intact in your original condition, although some of the people would have liked to have seen that happen. But it was a question of adaptation to the new world, changing world, and development of institutional means to deal with some of the problems that physicists could see they had, including a shrinking number of jobs. So I don't see how the Society could have maintained its pristine role as a publisher of abstracts and papers. One would have had to invent some other society in parallel if it had.

**Sopka:**

Are you in favor of the Physics Society taking stands on public issues, not necessarily right at the moment but as a policy if some issue arises in the next six

months or two years? Are you in favor of the Society going on record with a consensus, or is the idea of a consensus within the physics community impossible in view of the spectrum you mentioned?

**Purcell:**

I would not argue for any general inflexible principle. I think you have to look at each case. You have to ask first: "Is there a genuine consensus in the Society and not just a few people who want to make a lot of noise?" Had I said anything before about what my present interests in physics are, what I'm doing myself?

**Sopka:**

Not really. I think it would be valuable to have you discuss that.

**Purcell:**

Well, for many years now my own research interests have centered in some problems in astrophysics and then more recently in biophysics. These are very specific problems which I became intrigued with because of some of the elementary physics involved, and for want of anything else to do creatively, I've

been working on problems connected with the interstellar medium particularly and interstellar dust. This has been going on really now for many, many years. I've published a number of papers, but I can’t really say that the subject has been advanced very much by them.

**Sopka:**

Is this an area that's new to the astrophysical community as a whole and that you are becoming involved in at the same time that they are, or is it an established field where it's a new interest for you?

**Purcell:**

Well, it was an established field actually, and the problem that caught my attention is a problem that's 25 years old involving the alignment of the interstellar dust presumably by the interstellar magnetic field resulting in the polarization of starlight, which was observed as long ago as 1950. I just found that a number of interesting physics problems are presented by this. There's still no quite satisfactory theory of the interstellar dust alignment, and in a way this was also an outgrowth of my work on the 21 centimeter line and the involvement

through that with problems of the interstellar medium.

**Sopka:**

What techniques do you use for studying the interstellar medium? Is that radio astronomy?

**Purcell:**

No, not directly -- nearly all optical astronomy. Well, I'm sorry: radio astronomy as far as interstellar gases are concerned. The interstellar dust solid particles are completely transparent. As far as radio astronomy is concerned they have no effect whatever directly. But the interstellar dust is emerging now as a very important and critical component of the interstellar medium. For one thing, it is the catalyst for interstellar chemistry, at least for basic reaction of hydrogen molecule formation. And the original condensation of the medium for protostars and stars and planets, the dust may well be -- most likely is -- involved in a very important way. So I've been able to make some contributions to the subject, I think, but it's still a rather messy subject and one that I find as a physicist not terribly satisfying in that the observational data are

sufficiently meager and limited in their kind that one has really no way of getting rid of theories. There's a lot of speculation, and the speculation keeps churning around again and again, and you can't really weed out the nonsense.

**Sopka:**

Is this an area where the data can be expanded through the use of telescopes mounted on satellites?

**Purcell:**

Oh, very much so. In fact, there's been an enormous progress. That's one big change since I began dipping into the subject. The big important change has come from the use of these satellite telescopes -- OAO II and OAO III, Copernicus, have provided material that is just completely new. So there are now things that call out for explanations, and, in fact, I think already one could say that Copernicus has provided the material to demolish a number of theories, and things are looking up. And there's no question but what satellite telescopes are going to eventually be the means by which this subject is finally settled. There's no doubt about that.

**Sopka:**

I guess then from what you said just now and what you said earlier that your own assessment of the total space program is that this is a very worthwhile thing for the scientific world from a long-range point of view.

**Purcell:**

Oh, yes. In fact, the large space telescope, which unfortunately is now in financial support trouble and is being shot at from various sides, is certainly one of the most important projects we could possibly be engaged in at this time in science. There's just no question that it will broaden the astronomical horizons a major amount. I enjoy working on the interstellar medium simply because it gives me a kind of license to follow astronomy and at least listen in on the really sensational things that are going on in astronomy now.

**Sopka:**

The work that you have done -- has it been on your own or in collaboration with local astronomers or astrophysicists?

**Purcell:**

Well, it's been on my own to a considerable extent, but I have enjoyed lots of discussions. In particular, I suppose my major paper on the grain alignment was one done in collaboration with Professor Spitzer at Princeton, who is one of the leading astrophysicists of the interstellar medium and from whom I've learned a great deal.

**Sopka:**

Did you and he get together through a meeting of common interest or was there any other reason why you happened to collaborate with him?

**Purcell:**

Well, he had been writing related papers on this, and I had on my own adopted another approach, and we decided to get together and write a paper in which we brought the two different approaches together, and this was a very educational experience for me because Lyman Spitzer knows so much astronomy and I know so little. But I enjoyed it very much and I think it was a fairly definitive paper as of that stage of the development.

**Sopka:**

Are you associated with any of the activities here in Cambridge of the Harvard College Observatory and the Smithsonian Astrophysical Observatory?

**Purcell:**

Well, I've been involved for quite a long time, not in an active observing way but in a more administrative way, with what we call NEROC, the Northeast Radio Observatory Corporation. This runs the Haystack Radiotelescope, and I've been involved with all the people and one of the officers of the NEROC Corporation. And although I don't do any work at Haystack, I've followed what they do and I've been involved in all of the discussions and trying to get budgets and grants and so on. In running Haystack, for several years now we've been running it on an NSF grant as a radio observatory. It's a very remarkable instrument.

**Sopka:**

Where is Haystack located?

**Purcell:**

It's near Groton where MIT had the Westford Laboratory out there. I forget the name of the hill, but there are several radar antennas out there, and the Haystack antenna was originally a Lincoln Laboratory installation but it's now run as a purely scientific installation. NEROC is a corporation involving a lot of universities and colleges in this vicinity. Harvard and MIT are sort of the main, most active ones, but Yale and Brandeis and B.U. and a dozen or so are formally involved, and it's a laboratory to which any radio astronomer can apply for a time there for a project, and it's very busy and does very good work. It's a very active member of some of the long baseline interferometery networks, and works with the Goldstone antenna in California and with the 140-footer in Green Bank and with antennas in Russia and other places. So I have been involved in that work and also in the attempts to get a big antenna. This thing up here [picture on wall] was a proposal we made several years ago. We spent about $1 million of NSF money designing this, and then it was finally not approved for construction but it would have been enormous -- a 400-foot antenna. I was involved in the planning on that.

**Sopka:**

Does it appear that money will still continue to be available for the continuation and extension of this kind of activity from NSF and other sources?

**Purcell:**

Well, the big national project now in radio astronomy which is going on is the Very Large Array, which is being completed in New Mexico under the aegis of the National Radio Astronomy Observatory, and that's a couple of hundred million dollar project. And essentially we were competing with that. The decision was made not to fund a very large dish of this type but to fund a very large array at that time, which I think was the correct decision. And that's going together very well. It's proceeding. They've already got ten or twelve of the antennas finished. But radio astronomy is an extremely fruitful and lively subject now and could easily absorb and put to good use more money than the NSF has to spend on it. Money is very tight. But there's no shortage of new discoveries of both sort of local significance and cosmological significance. Interstellar chemistry is a big field now. Bill

Klemperer over here is in the middle of that figuring out all of the reactions that must be going on to make all these molecules.

**Sopka:**

That is quite exciting, to consider that you have a kind of laboratory situation way out there under conditions that you couldn't duplicate in the laboratory, and yet you can get the information about what's going on.

**Purcell:**

Right.

**Sopka:**

That would presumably indicate that there would be an expected flow of bright young minds into the field where there is such an active, exciting topic.

**Purcell:**

Yes, yes, although I'm not sure that the number of jobs … Well, radio astronomy doesn't take many observers to tie up a big telescope. Put it that way. The long baseline interferometry has an interesting

future. There's a project now to measure continental drift.

**Sopka:**

Oh, I didn't know about that.

**Purcell:**

Yes, it's so accurate now that one is on the verge of being able to measure the distance between two continents to half an inch; and if so, then you can see them move from year to year as they drift.

**Sopka:**

That's really exciting. Then your other interest now in biophysics is bringing you down out of space into…

**Purcell:**

Yes. Well, that grew out of my friendship and association with Howard Berg, who was here at Harvard as a Junior Fellow and a graduate student in physics and is now a biologist. In fact, he's chairman of the department of biology at the University of Colorado. I don't know that this subject will continue to offer opportunities for me, but I think at the

moment we seem to have found a little soft spot where the applications of rather elementary physics can clear up some of the questions that are relevant to the biological system, questions having to do with the physics of the bacterial environment. And I find it very interesting because I've always been puzzled by the fact that so small an organism as a bacterial cell-only a micron or two in size -- can carry on such a complicated life. And I think I'm beginning to see how at least the physics of that can be understood. Not to say that the important problems aren't biochemical -- they of course are. But I think the physicist going into a bacteriological problem has simply got to admit that what's going on is biology and chemistry and that just importing a little physics is not going to answer the important questions. But there are some questions that need to be taken care of that way.

**Sopka:**

Did you need to teach yourself some biology …?

**Purcell:**

Not very much because I'm looking at problems … It's rather like the astronomy. I didn't really… how

shall I say? I don't need to teach myself a lot of general astronomy, because the problem is fairly well defined. I, of course, have to rely on an astronomer in that case or a biologist in this case to define the problem for me and to say when my calculations are relevant or not relevant. I, of course, learned a little doing this, but very, very little.

**Sopka:**

Is this a quantum mechanical realm in biology when you're working with this?

**Purcell:**

Well, the quantum mechanics of course comes into the elementary chemical reactions, but the scale of phenomena that I'm concerned with here are not that. They're a somewhat larger scale, and the physics is classical physics of a very old-fashioned kind as far as the fluid is concerned, and then the part that's been interesting to me is the part that involves fluctuations and Brownian motion and things like that. In other words, the bacterial cell is small enough so that Brownian motion -- its own Brownian motion -- is a non-negligible movement, and it's dealing with a few enough molecules so that

fluctuations in the number of the molecules is not completely negligible. But still this little machine has got all kinds of wonderfully working parts to it. An easily stated important problem, of course, in that whole area is in the physics of the cell membrane. What is the cell membrane and what is the actual mechanism that controls its permeability to different things? Of course, there's an army of biologists working on that as the central problem in cell physiology, the properties of the membrane. And I haven't yet had any ideas for contributing to that. That would be much more important than what I've been doing.

**Sopka:**

Your recent comments then certainly highlight the aspect of the centrality of physics to a number of these other disciplines.

**Purcell:**

For me this is just kind of intellectual sport. I have no illusions that my personal contribution here is going to change the course of science at all, but it gives me a chance to keep my mind active, to be learning things, and to follow fields where there are

other extremely important developments going on and have some glimmer of what they mean. Howard Berg and others now really have their hooks on some of the behavioral aspects of bacteria, E. coli in particular; and I think a great deal is going to be learned about that. It's fascinating stuff. And from my sort of seat on the sidelines I can follow that, earning my way in by a little bit of elementary physics.

**Sopka:**

You've been able to witness during your professional career a different area of excitement of physics essentially in each of the decades.

**Purcell:**

Yes. Well, of course, the central excitement in physics is one I wish I could follow -- particles. We're in tremendous times in that. If I had some way of understanding what is going on there well enough to sort of take part in it, I would much rather do that than all this other stuff, because clearly that is fundamental. There's no question about it. And the cosmology, too, is fascinating. We're at this point in

the study of the universe. We've discovered the relic of the big bang.

**Sopka:**

Well, I gather then that although you're personally reaching the Harvard initial state of retirement, that you have lots of things which will keep you busy for a number of years.

**Purcell:**

I hope so, but, you know, I can't be sure. It's just a question of what will turn up next.

**Sopka:**

And also that your interest in teaching will continue.

**Purcell:**

Yes, but I think one has to be very careful to recognize the time when one should stop teaching.

**Sopka:**

Speaking of teaching, we missed one topic that we were going to have you comment upon, and it goes back in time to the development of PSSC. Would

you want to make a few comments before we reach the end of the tape on that?

**Purcell:**

Yes. PSSC was one of the many times in my life when I have been enlisted under the banner of Jerrold Zacharias. In most of those times I was glad afterwards that I had, and this was certainly the case in PSSC. We wanted to do something about the teaching of physics in high schools, and whether we did any good in the end or how much good we did is still a matter for debate, but we certainly loosened the situation up. I had a lot of fun. We made movies, high school movies, and I made two of those and learned a lot about that. I'm certainly glad I took part in that. In a way it was also a forerunner of the Berkeley physics course, in the conception of which Zacharias also took part, although the real leader in that was Charles Kittel. And ten or twelve years ago I put a lot of effort into my contribution to the Berkeley physics course in the shape of Berkeley Volume II, and I'm very glad I did that because I just have taken a lot of satisfaction in having written what I think is a good book. And the freedom to do it and the inspiration to do it and the support while

doing it came from the Berkeley group. Really it was one of the memorable experiences of my life, that Berkeley chapter. The book ought to be revised, but I somehow haven't gotten very far toward revision of that, and I don't know whether that will ever take place. As I look at what I really ought to be doing as opposed to what I enjoy doing or end up doing, it's probably true that I ought to be revising Berkeley Volume II.

**Sopka:**

That's the section on electricity and magnetism?

**Purcell:**

Yes, that's on electricity and magnetism. Of course, I've never received any royalties for that book. None of the people did. The NSF supported the project, and all the royalties go to the U.S. government, so although the book has sold very well and is very widely used -- not only here but also in Europe -- it has been translated into seven or eight languages -- I never made a nickel out of it. We were paid a lump sum for writing it, a rather small sum.

**Sopka:**

Well, I hope that you decide to revise it.

**Purcell:**

Yes, I think it ought to be revised, but whether I can actually organize myself to do that and complete that job is… in a nutshell, that's what I ought to be doing. There's no question about it. I might even put it into MKS units. Perish the thought.

**Sopka:**

What units is it written in now?

**Purcell:**

Good old Gaussian CGS, part of the counter-revolution. But in Europe now it's not even legal to use a book that is not in MKS.

**Sopka:**

Oh, I didn't realize that.

**Purcell:**

Oh, yes. I don't think you could adopt such a book if you were a teacher in a German or European university…

**Sopka:**

Well, I thank you very much. I think we've covered…

**Purcell:**

We've certainly covered more than anybody wants to read.

**Sopka:**

Oh, no, I don't think you should say that at all. I'm very pleased with the wide range of topics we've covered. I can't think of anything further to ask you about. We'll be in touch.

**Purcell:**

All right. Okay.

# Interview Session – 4

Undergraduate in Electrical Engineering at Purdue. LH had just come to Purdue to a zero department and quickly made it into a research place. Wanted to take a course in independent physics research. Got hooked on physics at that time. Worked with Walerstein and later Hugh Yearian. Yearian was not a PhD yet but was extremely experienced. Worked with Yearian in his senior year. Did not have to report directly to Lark-Horovitz. "The Lark ran it as sort of a European style with him at the top of the pyramid and gave orders to everybody down the way." First published paper was with Lark-Horovitz on making thin films for electron diffraction. Carried out LH idea. Graduated in electrical engineering. Changed to physics, LH helped him get an exchange scholarship to Germany. Sure that LH letter helped him get into Harvard. Purdue was an interesting place at that time. LH was giving courses in topics that were in the very forefront of experimental physics at that time. Very quick with a lot of things, but he wouldn’t write things up and publish them. Doesn't know why. He was very imaginative, very quick and impatient with the graduate students. LH

went to Berlin to visit in 1933 and talked to a man named Rupp. Story of LH and the making of thin films on rock salt. Illustrates LH desire to try new things even if they were possibly wrong. Some of the graduate students lived in the basement.

Purcell would go to Walerstein or Yearian with questions. LH had the intellectual tradition of a liberal Viennese Jew. Loved music. Made a little island of Central European Judaism in West Lafayette Indiana.

PhD in 1938. Then helped build the Harvard cyclotron. Did not do any nuclear physics experiments with it. Wanted to try an air core betatron.

Began at RL in Dec. of 1940. People at Harvard were already there. Bainbridge and Rabi were involved. Asked Purcell to come up to a meeting at Harvard. Purcell worked there part time until the term was over and then went full time in January. Lawrence and some people at MIT were involved in setting it up.

First work was in the magnetron group along with Ramsey. Does not recall if he had a choice in where

to work. Were magnetron, receiver, pulser and antenna groups at that time. First assignment for RL was to make an airborn 10 cm radar for a night fighter. Had to make a pulser. Just tested magnetrons, did not have the facilities for making them. The magnetrons came from Bell. Then came the problem of the Transmit-Receive switch. [It was very desirable from the standpoint of limitations on size of radar sets for airplanes that the sets use the same antenna for sending and receiving. But the powerful outgoing signal would burn out the sensitive crystal rectifier if it was in the circuit when the pulse went out. They needed a switch that would allow the receiving circuit to be switched in and out in fractions of a second. They solved the problem by developing a gas discharge tube that would arc and therefore short out part of the circuit.] Zacharias was in the antenna group at the beginning. Lists other people who were there.

All in two big rooms, but in a well-defined group. Reported to Rabi. Spent two years in the roof building in Group 41 Fundamental Developments. Ramsey was first leader of 41, then Purcell took over. Also involved with Propagation, head of the group for a while. Purcell's group was doing 3 cm.

and later K band. Did much more basic research, and being a free wheeling group, when they made models of 3 cm work they were free to tryout ideas. Had Stewart Foster in his group. Brilliant inventor, but couldn't fit in with the antenna group. Description of Group under Otto Halpern. Purcell's group was a very small and informal group. Had Dicke and Berringer in the group. Let them do what they wanted. Story of mortar shell tracking by radar and the man who founded Tektronix.

Oscilloscopes developed during the war. Needed faster phosphors. British started making the oscilloscope tubes with a flat face. Thinks the British did the first work on dark-trace tubes: the "dark face tube and the flat face tube." Remembers during his trip to England in 1942, that he went to Bristol to see a man named Sutton who was a professor at Bristol and had been doing defense work in his lab. He was making flat face tubes very simply. He had lot of high school kids trained as technicians to make these tubes. Purcell commented on his return to the Rad Lab, how simple it seemed for Sutton to make these tubes. Dark tubes were made with the halides and erased with infra-red. Project never did succeed or turn out to be important.

Fiasco in Purcell's group. 3 cm airborn interceptor conical scanner for aircraft was too complicated to work.

Needed a gyrator circulator. Needed an insulating ferromagnet. Stuck with reciprocity theorem during the war. Solid state thing that came too late. More important development was permanent magnetic material.

In 1940 knew the magnetron needed a permanent magnet for field units. Art of designing permanent magnets was entirely unknown. Bainbridge came across an article in the Philips Technical Journal in 1939 or 40 that told all you need to know about permanent magnets. Then at Bell Labs with Bozorth more was done. Gradually AlNiCo 5 became available. Needed stronger fields for shorter wavelength.

Screwdriver effect. Screwdrivers touching the magnet would demagnetize the alnico. Modern position in magnetic material came during the war. Also didn't have polyethyline insulation in coax cable.

British even started that. First plants for polyethyline at that time. At that time had to use beads to insulate the coax cable. Microwaves for physics research. Microwave spectroscopy, paramagnetic resonance. Nuclear magnetic resonance work was done at 30 Mc because that was the radar frequency and the equipment was available. Microwave ovens also, developed after the war, by Raytheon who was making magnetrons in large quantities. Problems with microwave ovens and mode stirrers. Came directly from their terrible mistake about K band, which they discovered when they tried to measure water vapor absorption. [Water vapor absorbs strongly at 1.25 cm, thus rendering the K band radar useless when water was present.] Dicke tried to make measurements of the absorption with his radiometer.

Applications of Rad Lab work. Precision time measurement came from the Loran and Shoran work. Gas discharge tubes for the TR problem. Understood microwave circuits very well after three years, also magic tees. Harp project under Otto Halpern.

Manufacture of magnetrons and tubes advanced. Advances were in manufacturing practices for putting hard vacuum devices together. Only U.S. made metal tubes and double tubes. British never made double triodes or anything. They always made the tubes out of glass. There used to be good natured fights between British and Americans. Over who had the fewest tubes in their receivers. British counted American double tubes as two tubes, or valves. Other differences in relations with British industry. Rad Lab people were used to dealing with places like Bell Labs, Raytheon etc. Rad Lab physicists on an even basis with the Bell Labs people. Purcell's opposite number in England was a university scientist who had to deal with GEC Wembley for example. University scientist treated the British manufacturers like tradesmen. "One felt this sort of social difference." Didn't have the mutual give and take due to the way British industry was set up. British engineers were not trained at a place like MIT; they had come up as apprentices in industry. Severe handicap for them.

Pulse forming line was simultaneous invention. And rectifiers. Skinner discovered tapping.

Working at the Rad Lab was the foundation of Purcell's career. Everything he did after came from the RL. Had Bob Dicke in his group inventing the radiometer, forced by the k band problem to investigate the absorption at resonance of water vapor. Thrown together with Ramsey and Zacharias who had worked with Rabi in his molecular beam lab before the war. Rabi was his superior. Learned about molecular beam resonance, and signal to noise, so when deciding after the war which experiments to do with the new equipment, they knew if they could do them or not.

So in the NMR, done at the Rad Lab yet, they could calculate how big the signal should be. And about saturation on the 21 cm experiment on hydrogen. First radio astronomy experiment used a plywood horn that he knew how to build. Had a very sensitive receiver in the room. Knew how everything should turn out, except if they would see the 21 cm line. "My whole life as a physicist, basically, traces to that." First nuclear resonance was done by Pound and Torrey. Pound had a B.A. from Buffalo, knew more about microwave amplifiers than anyone else. Had a parametric amplifier working on his bench.

Torrey looked out for the Purdue contract. Torrey had worked with Rabi.

Work style at the Rad Lab. Memories of England in 1942.

Torrey was in group 53. Purcell had no subcontractors of its own. Reflex klystron tubes at Oxford and Raytheon. Informal contacts with industry was getting out of hand. Steering Committee tried to make the scientists stop by decreeing that all letters to Bell Labs had to be from DuBridge to Bown. So they wrote letters and signed DuBridge's name to them. Military relations were good on the whole.

New people were assigned to groups by Wheeler Loomis. Bob Dicke was assigned to Purcell's group. Spent his first afternoon there trying to tune a local oscillator. Lab Policy on fundamental research? Didn't need a policy. People concerned with the job they had to do, but they were mostly physicists, who knew something might be found by accident. Vic Neher made the first I cm local oscillator. Guy Stever, later science advisor to Nixon, was Neher's assistant. He used the 1 cm to look at absorption of

ammonia in a waveguide. Rabi didn't think some of the propagation work was useful, but it turned that anomalous propagation was important to the Navy.